PHILOSOPHICAL TRANSACTIONS.

PART II.

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OF THE

ROYAL SOCIETY

OF

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VOL. LXXI. For the Year 1781.

PART II.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

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PHILOSOPHICAL TRANSACTIONS.

XV. *New Experiments upon Gun-powder, with occasional Observations and practical Inferences; to which are added, an Account of a new Method of determining the Velocities of all Kinds of Military Projectiles, and the Description of a very accurate Eprouvette for Gun-powder.* By Benjamin Thompson, *Esq; F. R. S.*

Read March 29, 1781.

THESE experiments were undertaken principally with a view to determine the most advantageous situation for the vent in fire-arms, and to measure the velocities of bullets, and the recoil under various circumstances. I had hopes also of being able to find out the velocity of the inflammation of gun-powder, and to measure its force more accurately than had hitherto been done. They were begun in the month of July in the year

1778, at Stoneland Lodge, a country ſeat of Lord GEORGE GERMAIN's, and I was aſſiſted by the reverend Mr. BALE, rector of Withyham, who lives in the neighbourhood.

The weather proved remarkably favourable for our experiments, being ſettled and ſerene, ſo that the courſe of them was never interrupted for a whole day by rain or by any accident. The mercury in the barometer ſtood in general pretty high, and the temperature of the atmoſphere was very equal, and moderately warm for the ſeaſon. In order that each experiment might, as nearly as poſſible, be under ſimilar circumſtances, they were all made between the hours of ten in the morning and five in the afternoon: and after each diſcharge the piece was wiped out with tow till all the inſide of the bore was perfectly clean, and as bright as if it had juſt come out of the hands of the maker; and great care was taken to allow ſuch a ſpace of time to elapſe between the firings, as might render the heat of the piece nearly the ſame in every experiment.

A deſcription of the apparatus.

The barrel principally uſed in theſe experiments was made by WOGDON, one of the moſt famous gunſmiths in London; and nothing can exceed the accuracy with which it is bored, or the fineneſs of the poliſh on the inſide. It is made of the very beſt iron, and, agreeably to Mr. ROBINS's advice, I took care to have it well fortified in every part, that there might be no danger of its burſting. Its weight and dimenſions may be ſeen in the table of the weight and dimenſions of the apparatus, p. 247.

Fig. 1. Repreſents a longitudinal ſection of a part of the barrel, with the apparatus firſt made uſe of for ſhifting the vent from one part of the chamber to another, or rather for moving the

the bottom of the chamber further from, or bringing it nearer to, the vent, in order that the fire might be communicated to the powder in different parts of the charge.

a, *b*, repreſent the lower part of the barrel.

c, is the breech-pin, which is perforated with a hole four-tenths of an inch in diameter, the axis of which coincides with the axis of the bore.

Into this hole the ſcrew *b*, *n*, about four inches in length, is fitted; to the end of which, *n*, that paſſes up into the bore, is fixed a piſton *o*, *p*, which, by means of collars of oiled leather, is made to fit the bore of the piece very exactly. The end of the piſton *p*, neareſt the muzzle, is of braſs, and forms a moveable bottom to the bore, which by turning the ſcrew *b*, *n*, by means of the handle *m*, is brought nearer to, or removed further from, the fixed vent *v*, by which means the powder is lighted at any aſſignable diſtance from the bottom of the charge.

But the length of the bore being altered by moving the piſton, which occaſioned a ſmall inaccuracy, and ſome inconvenience attending the apparatus, it was laid aſide, and another repreſented by fig. 2. was ſubſtituted in the room of it.

a, *b*, is a ſection of part of the barrel as before, and *c* is the breech-pin, which being perforated with a ſmall hole through its center receives the ſcrew *f*, *g*, which is about two-tenths of an inch in diameter, and four inches long. This ſcrew being perforated with a very ſmall hole, ſerves to convey the fire into the chamber of the piece, and by ſcrewing it further up into the bore, or drawing it backwards, the fire is communicated to different parts of the charge.

But this method being found to be not intirely free from inaccuracies and inconveniencies, a third was ſubſtituted in the

room of it, which was found to anſwer much better than either of the preceding.

The end of the bore was now firmly cloſed by a ſolid breech-pin *p*, fig. 3. and three vent holes *m*, *n*, and *o*, were made in the barrel; one of them, *m*, even with the bottom of the bore, and the other two at different diſtances from it. Any two of theſe vent holes, as *n* and *o* for inſtance, being cloſed up by ſolid ſcrews, a perforated ſcrew, or vent tube *v*, was ſcrewed into the third, which ſerved to contain the priming, and to convey the fire to the powder lodged in the bore of the piece.

Sometimes a longer vent-tube, repreſented by fig. 4. was made uſe of; which, paſſing through the powder in the chamber of the piece, communicated the fire immediately to that part of the charge that lay in the axis of the bore.

Another vent-tube alſo was uſed occaſionally, which differs in many reſpects from both thoſe that have been deſcribed. It is ſo conſtructed as to convey the fire to the charge; but, as ſoon as the powder in the chamber of the piece begins to kindle, and the elaſtic fluid to be generated, the vent is firmly cloſed by a valve, and no part of the generated fluid is permitted to eſcape. This I ſhall call the *valve-vent*, and it is repreſented by fig. 5. upon an enlarged ſcale, that the parts of it may appear more diſtinct.

a, *b*, is a longitudinal ſection of a ſmall portion of the ſolid ſide of the barrel.

c, *d*, is the vent-tube, which is in all reſpects like the ſhort vent-tube commonly made uſe of, except only that in this the end of the vent-hole (*c*) which goes into the chamber is enlarged in the form of the wide end of a trumpet or funnel.

To this enlarged aperture the valve, *v*, is accurately fitted, and by means of the ſmall ſtem or tail, *t*, which is fixed to the valve,

valve, and which passes up through the vent-hole, and is connected with the spring S, the valve is pressed, or rather drawn into its place, and the vent is closed. The stem of the valve was at first made cylindrical; but, in order to make way for the priming to pass down to the valve, one-half of its substance was taken away, as is represented in the figure.

When this vent is primed, the space between the vent hole and the stem of the valve is filled with fine-grained powder, and the valve is gently opened by pressing upon the end of the stem till one or more grains of powder lodge themselves between the valve and the aperture; which preventing the valve from closing again, a small opening is left for the passage of the flame into the chamber of the piece: therefore, when the priming is lighted, the fire passing down the vent, and entering the chamber, inflames the charge, and the small grains of powder that were lodged between the valve and the aperture being destroyed by the flame in its passage through the vent, the valve immediately closes, and prevents the escape of any part of the elastic fluid generated by the inflammation of the powder in the chamber of the piece. The pressure of this fluid upon the valve assists the action of the spring, by which means the valve is more expeditiously and more effectually closed.

The valve was very accurately fitted to the aperture by grinding them together with powdered emery, and afterwards polishing them one upon the other. And it is very certain, that no part of the elastic fluid made its escape by this vent; for, upon firing the piece, there was only a simple flash from the explosion of the priming, and no stream of fire was to be seen issuing from the vent, as is always to be observed when a common vent is made use of, and in all other cases where this fluid finds a passage.

In order that every part of the apparatus employed in these experiments might be as perfect as possible, all the more delicate parts of it were executed by Mr. FRASER, mathematical instrument-maker in Duke's Court, St. Martin's Lane, and, among the rest, all the contrivances just described relative to the vent.

The velocities of the bullets were determined by means of a pendulum, according to the method invented by Mr. ROBINS.

The pendulum I made use of (fig. 6.) is composed of a circular plate of hammered iron (*a*), 13 inches in diameter, and 0,65 of an inch thick, to which is firmly fastened a bar of iron (*b*, *c*) 56,5 inches in length, 2,6 inches broad, and half an inch in thickness, by which it is suspended by means of two pivots (*d*, *e*) at the end of the bar (*c*), and at right angles to its length. These pivots being very accurately finished, and moving on polished grooves, which were kept constantly oiled to lessen the friction, the vibration of the pendulum was very free, as appeared by the great length of time its vibrations continued after it had been put in motion, and was left to itself. To the circular plate of the pendulum, targets of circular pieces of wood of different thicknesses were fixed, which in the course of the experiments were often spoiled and replaced: and, in order to mark the weight and dimensions of the pendulum in each experiment, the pendulums are numbered according to the different targets that were made use of; and the weight and dimensions of each pendulum are set down in a table at the end of the description of the apparatus.

The target of the pendulum N° 1. was made of a circular piece of elm-plank, 3¼ inches thick, and equal in diameter to the iron plate of the pendulum to which it was fixed; but this target being too thin was very soon ruined.

The

The pendulum N° 2. was furniſhed with two targets, which were circular pieces of very tough oak-plank, near five inches thick, placed on oppoſite ſides of the plate of the pendulum, and firmly fixed to it by ſcrews, and to each other by iron ſtraps. When one of theſe targets was ruined, the pendulum was turned about, and the other was made uſe of. This pendulum laſted from experiment N°9. to experiment N°39. when it was ſo much ſhattered as to be rendered unfit for further ſervice.

The pendulum N° 3. was like the pendulum N° 2.; only, inſtead of oak, elm-plank near ſeven inches in thickneſs was made uſe of for the targets. This pendulum ſerved from experiment N° 40. to experiment N° 101. inclusively.

But finding that targets made of planks of the toughest wood were very ſoon ſhattered to pieces by the bullets, I compoſed the pendulum N° 4. in a different manner. Inſtead of circular pieces of plank, ſolid cylinders of elm-timber were made uſe of for the targets, ſo that the bullets now entered the wood in the direction of its fibres. Theſe cylinders are 13 inches in diameter, and about 5½ inches in length, hooped with iron at both their ends to prevent their ſplitting, and firmly faſtened to the plate of the pendulum, and to each other by four iron ſtraps. This pendulum laſted till the experiments were finiſhed. It is ſtill in being, and appears to be very little the worſe for the ſervice it has undergone.

Fig. 7. ſhews the two ends of the pendulum upon a large ſcale, together with the hooks or grooves by which it was ſuſpended.

a, *b*, is the bar of the pendulum, which is ſeen broken off, as there is not room to ſhew the whole of its length.

c, *d*, are the pivots by which it was ſuſpended.

e, is

e, is the circular plate of the pendulum, to which

f, *g*, two circular targets, are faſtened by ſcrews, and by means of the iron ſtraps, 1, 2, 3, 4, which are nailed to the edges of the targets.

h, *k*, are the hooks which ſerved inſtead of grooves to receive the pivots, *c*, *d*, of the pendulum.

The hooks were firmly fixed to the horizontal beam R. S. which ſupported the whole apparatus by means of three ſcrews *m*, *n*, *o*, which paſſed through three holes in the plate that connects the two hooks. When the hooks were faſtened to the beam, the middle ſcrew, *n*, was firſt put into its place, and the pendulum was allowed to ſettle itſelf in a poſition truly perpendicular, after which the grooves were immoveably fixed by means of the ſcrews *m*, *o*.

The chord of the arc, through which the pendulum aſcended in each experiment, was meaſured by a ribbon, according to the method invented and deſcribed by Mr. ROBINS.

The recoil was meaſured in the following manner. The barrel was ſuſpended in an horizontal poſition (and nearly in a line with the center of the target) by two ſmall pendulous rods, 64 inches in length, and 25,6 inches aſunder; which being parallel to each other, and moving freely upon poliſhed pivots about the axes of their ſuſpenſion, and upon two pair of trunnions that were fixed to the barrel, formed, together with the barrel, a compound pendulum; and from the lengths of the vibrations of this pendulum, the velocity with which the barrel began to recoil, or rather its greateſt velocity, was determined.

But in order that the velocity of the recoil might not be too great, ſo as to endanger the apparatus when large charges were made

made use of, it was found necessary to load the barrel with an additional weight of more than 40 lbs. of iron.

This additional weight of iron, which I shall call the *gun carriage*, as it was so constructed as to serve as a carriage to the barrel, is composed of a bar of hammered iron 28 inches in length, 2,6 inches broad, and half an inch in thickness, which is bent in the middle of its length in such a manner, that its two flat sides or ends are parallel to each other, and distant asunder two inches. In the middle of this bar where it is bent is a hole in the form of an oblong square, which, receiving the end of the breech-pin, supports the lower end or breech of the barrel. The other end of the barrel is supported and confined in the following manner. A ring or hoop of iron, near half an inch thick, and two inches in diameter, is placed in a vertical position between the parallel sides of the bar, and near its two ends, and firmly fixed to them by screws. The barrel passing through the middle of this ring is supported upon the ends of three screws, which passing through the ring in different parts of its circumference all point towards its center.

The carriage, together with the barrel, was suspended by the pendulous rods by means of two pair of polished trunnions that are fixed to the outside of the carriage. They are placed in an horizontal line perpendicular to, and passing through, the axis of the bore.

Fig. 8. represents the barrel fixed to the carriage.

a, *b*, *c*, is the bar of iron which forms the carriage seen edge-ways.

z, *z*, *z*, *z*, are the trunnions by which it was suspended.

d, *e*, is the barrel in its proper place.

p, is the breech-pin, which passing through a hole in the middle of the bar, *a*, *b*, *c*, supports the end, *e*, of the barrel; and

n, is the ring that supports the end, *d*, of the barrel.

Fig. 9. repreſents a perpendicular ſection through the line 2, 2, fig. 8. and in a line perpendicular to the length of the barrel.

This figure is deſigned to ſhew the manner in which the muzzle of the piece was ſupported and confined in the ring *n*, fig. 8.

a, *c*, are the two ends of the bar that are ſeen cut off.

n, is the ring, and

o, *p*, are the ſcrews by which it is faſtened to the two parallel ſides of the bar, the ends of which form the trunnions 2, 2, fig. 8.

d, is a tranſverſe ſection of the barrel, and

r, *s*, *t*, are the three ſcrews by which the barrel is ſupported and confined in the center of the ring.

Fig. 10. is the ſame as fig. 9. but upon a larger ſcale.

Fig. 11. repreſents the two ends of one of the pendulous rods by which the barrel was ſuſpended; and fig. 13. ſhews the ſame ſeen ſideways.

a, *b*, is the rod which is ſeen broken off.

c, *d*, are the pivots by which it was ſuſpended by a pair of hooks or grooves that were faſtened to an horizontal beam, in the ſame manner as the pendulum for meaſuring the velocities of the bullets was ſuſpended.

e, *f*, are the hooks which receive the trunnions that are fixed to the carriage.

The dimenſions of every part of this apparatus may be ſeen in the table, p. 242.

The chord of the arc through which the barrel aſcended in its recoil was meaſured by a ribbon, and the lengths of thoſe chords, expreſſed in inches and decimal parts of an inch, are ſet down in the tables. The method of computing the velo-

city of the recoil from the chord of the arc through which the barrel aſcended, is too well known to require an explanation; and it is alſo well known, that the velocities are to each other as the chords of thoſe arcs. The lengths of thoſe chords, therefore, as they are ſet down in the tables, are, in all caſes, as the velocities of the recoil.

The powder made uſe of in theſe experiments was of the beſt kind, ſuch as is uſed in proving great guns at Woolwich. A cartridge, containing 12 lbs. of this powder, was given to me by the late General DESAGULIERS of the Royal Artillery, and Inſpector of Braſs and Iron Ordnance; who alſo, in the politeſt manner, offered me every other aſſiſtance in his power towards completing the experiments I had projected, or in making any others I ſhould propoſe that might be uſeful in the proſecution of my inquiries.

This powder was immediately taken out of the cartridge, and put into glaſs bottles, which were previouſly made very clean and dry; and in theſe it was kept carefully ſealed up till it was opened for uſe. When it was wanted for the experiments, it was weighed out in a very exact balance, with ſo much attention, that there could not poſſibly be an error in any inſtance greater than one quarter part of a grain. The bottles were never opened but in fine weather, and in a room that was free from damp, and no more charges of powder than were neceſſary for the experiments of the day were weighed out at a time. Each charge was carefully put up in a cartridge of very fine paper, and theſe filled cartridges were kept in a turned wooden box, that was varniſhed on the inſide as well as the outſide, to prevent its imbibing moiſture from the air.

The paper of which theſe cartridges were made was ſo fine and thin, that 1280 ſheets of it made no more than an inch in

thickneſs, and a cartridge capable of containing half an ounce of powder weighed but three quarters of a grain.

The cartridges were formed upon a wooden cylinder, and accurately fitted to the bore of the piece, and the edges of the paper were faſtened together with paſte made of flour and water.

When a cartridge was filled, the powder was gently ſhaken together, and its mouth was tied up and ſecured with a piece of fine thread; and when it was made uſe of it was put entire into the piece, and gently puſhed down into its place with the ramrod, and afterwards it was pricked with a priming-wire thruſt through the vent, and the piece was primed; ſo that no part of the powder of the charge was loſt in the act of loading, as is always the caſe when the powder is put looſe into the barrel: nor was any part of it expended in priming; but the whole quantity was ſafely lodged in the bottom of the bore or chamber of the piece, and the bullet was put down immediately upon it, without any wadding either between the cartridge and the bullet, or over the bullet.

The bullets were all caſt in the ſame mould, and conſequently could not vary in their weights above two or three grains at moſt, eſpecially as I took care to bring the mould to a proper temperature as to heat before I began caſting: and when leather was put about them, or other bullets than thoſe of lead were made uſe of, the weight was determined very exactly before they were put into the piece.

The diameter of the bullet was determined by meaſurement and alſo by computation from its weight, and the ſpecific gravity of the metal of which it was formed; and both theſe methods gave the ſame dimenſions very nearly.

The apparatus was put up for making the experiments in a coach-houſe, which was found very convenient for the purpoſe,

as the joists upon which the floor over head was laid afforded a firm and commodious support for suspending the pendulum and the barrel, and the walls and roofs of the building served to screen the apparatus, which otherwise might have been discomposed by the wind, and injured by the rain and dews. A pair of very large doors, which formed the whole of one end of the room, were kept constantly open during the time the experiments were making, in order to preserve the purity of the air within the house, which otherwise would have been much injured by the smoke of the gun-powder; and that, in all probability, would have had a considerable effect in lessening the force of the powder, and vitiating the experiments. In order still further to guard against this evil, the barrel was placed as near as possible to the door, and the pendulum was hung up at the bottom of the room.

Fig. 12. represents the apparatus as it was put up for making the experiments.

a, *b*, is the barrel with its carriage, suspended by the pendulous rods *c*, *d*, and

R, is the ribbon which served to measure the ascending arc of its recoil.

P, is the pendulum, and

r, the ribbon that measured the arc of its vibration.

The distance from the mouth of the piece to the pendulum was just 12 feet.

A table

A table ſhewing the weights and dimenſions of all the principal parts of the apparatus.

Of the barrel.

	Inches.
Length	44,7
Length of the bore from the muzzle to the breech-pin	43,45
Diameter of the bore	0,78
Thickneſs of metal at the lower vent	0,36
Thickneſs of metal at the muzzle	0,1

Weight of the barrel, together with the ſolid breech-pin, and the vent-ſcrews and vent-tube, 6 lbs. 6 oz.

Of the gun carriage.

Length	28,4
Diſtance between the two pair of trunnions	25,6
Diameter of each trunnion	0,25

Weight 40 lbs. 14 oz.

Of the rods by which the carriage was ſuſpended.

Length from the axis of ſuſpenſion, or center of the pivots, to the center of the trunnions of the gun carriage, 64 inches.

Weight of each rod, 1 lb. 4 oz.

Total weight of the barrel and its carriage, together with the allowance that was made for the weight of the rods by which it was ſuſpended, 48 lbs.

N. B. This was its weight from experiment N° 3. to experiment N° 123. incluſive.

Of

Of the bullet.

Diameter 0,75 of an inch.
Weight in lead 580 grains.

Of the pendulum.

	Inches.
Total length of the pendulum from the axis of ſuſpenſion to the bottom of the circular plate	69,5
Diameter of the circular plate to which the targets were faſtened	13,-
Diſtance between the ſhoulders of the pivots	3,8
Diameter of the pivots	,27

Weight of the iron part of the pendulum 47 lb. 4 oz.

Of the pendulum with the targets fixed to it, as it was prepared for making the experiments, and numbered.

	Total length to the ribbon.	Diſtance from the axis of ſuſpenſion.		Total weight of iron and wood.
		To the center of gravity.	To the center of oſcillation.	
	Inches.	Inches.	Inches.	lbs. oz.
Pendulum N° 1	69,25	50,25	58,45	57 0
—— N° 2	69,5	54,4	59,15	82 4
—— N° 3	——	55,62	60,23	100 12
—— N° 4	——	54 6	59,18	88 4

N. B. The meaſure is Engliſh feet and inches, and the weight is avoirdupois.

Having

Having now gone through the deſcription of all the principal parts of the apparatus, I ſhall proceed to give an account of the experiments. And as it may be ſatisfactory to the Society to ſee the method of conducting theſe enquiries, as well as the reſult of them, I ſhall firſt give a table of the experiments in the exact order in which they were made, together with my original remarks; I ſhall then make ſuch general obſervations as may occur: and afterwards I ſhall ſelect, combine, and compare them, in the manner which beſt anſwers the different purpoſes to which I ſhall apply them.

General

General table of the experiments.

In the two first experiments the barrel was fixed to a carriage (that has not been described) which, together with the barrel and rods by which it was suspended, weighed only 23*l*.

Length of the bore of the piece 43,5 inches.
Weight of the bullet 580 grains.

The pendulum, N° 1.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	The bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	In.	Ft. in Sec.	
N° 1	208	1,8	0,	13,2	64,5	33,5	1267	First day.
2	..	..	,5	14,5	...	36,5	1399	

This gun carriage being found to be too light, the other, described, and represented fig. 8. was substituted in the room of it.

Order of the experiments.	The charge of powder. Weight.	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	The bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N° 3	208	1,8	0	12,6	65,	17,8	1213	Second day.
4	..	..	,5	——	——	18,5	—	The pendulum gave way.
5	..	..	0	——	——	38,68	—	4 bullets were fired at once.
6	..	..	,5	——	——	38,48	—	Ditto.
7	..	..	0	——	——	6,1	—	Without any bullet.
8	416	3,6	..	——	——	16,5	—	Ditto.
9	208	1,8	0	8,5	65,	17,69	1281	Pen. N° 2; very fair; 3d day.
10	104	,9	..	5,2	65.25	10,18	782	
11	310	2,7	0	9,6	64,6	24,69	1459	
12	..	..	1,22	10,1	65,	24,95	1527	
13	..	..	2,65	11,85	64,75	24,9	1801	The powder was lighted by the long vent-tube (fig. 4.).
14	..	..	..	10,9	65,25	. . .	1646	
15	330	2,9	2,65	10,9	61,5	26,2	1748	
16	..	..	..	13,25	63,5	. . .	2060	
17	330	2,7	2,65	. . .	. . .	12,7	..	The barrel very much heated.
18	..	2,9	0	10,4	63,5	26,3	1619	
19	..	..	..	. . .	63,	26,4	1633	
20	165	1,45	0	6,8	62,2	14,73	1084	
21	..	..	..	6,85	. . .	14,2	1093	
22	..	..	1,32	6,7	. . .	14,8	1071	
23	..	..	..	6,3	60,6	14,58	1035	The short vent-tube (v, fig. 3.) was made use of.
24	..	..	..	7,5	61,5	14,68	1142	

In order to determine how much of the force of the powder was lost by windage and by the vent, oiled leather was fastened round the bullet, so that it now accurately fitted the bore of the piece; and in the five experiments, from N° 35. to N° 39. inclusive, the valve-vent was made use of.

Weight of the bullet, together with the leather in which it was enveloped, 603 grains.

Order

Order of the experiments.	The charge of powder. Weight.	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	The bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N°								
25	165	1,45	0	6,8	65,	14,95	1004	Fourth day.
26	..	..	..	7,8	...	15,6	1153	
27	..	..	..	8,05	...	16,15	1192	
28	330	2,9	..	10,2	63,	26,	1559	
29	..	..	2,6	...	64,	28,1	1536	
30	165	3,2	..	5,9	62,4	13,2	914	
31	..	1,45	1,3	6,65	62,6	15,15	1027	

Finding that the blaſt of the powder always reached as far as the pendulum, when large charges were made uſe of, and ſuſpecting that this circumſtance, together with the impulſe of the unfired grains, might in a great meaſure occaſion the apparent irregularity in the velocities of the bullets; to remedy theſe inconveniences, a large ſheet of paper of a moderate thickneſs was ſtretched upon a ſquare frame of wood, and interpoſed as a ſcreen before the pendulum at the diſtance of two feet from the ſurface of the target.

Two reaſons conſpired to induce me to prefer this method of preventing the impulſe of the flame upon the pendulum to the obvious one of removing the pendulum further from the mouth of the piece; the firſt was, that I was unwilling to increaſe the diſtance between the barrel and the pendulum, leſt the reſiſtance of the air might affect the velocities of the bullets; and the ſecond, which I confeſs did not operate leſs ſtrongly than the firſt, was, that the length of the houſe did not admit of a greater

distance, and I was unwilling to expose any part of the apparatus in the open air.

But the screen was found to answer perfectly well the purpose for which it was designed, and it was continued during the remainder of the experiments, the paper being replaced every third or fourth experiment.

The experiments continued.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
32	165	1,45	0	5,45	63,	15,45	839	Not leathered; weight of the bullet and wad 603 grs. In exp. N° 32. no less than 40 large grs. of unfired powder were driven through the screen.
33	..	..	..	...	...	12,65	839	
34	..	..	..	7,9	...	15,45	1217	In these 6 experiments the bullets were leathered, and the powder was lighted by the valve-vent.
35	..	..	..	7,	60,25	15,25	1129	
36	..	..	..	7,4	62,	16,3	1161	
37	..	..	1,3	8,	61,	17,9	1277	
38	290	2,6	2,6	9,	58,6	23,5	1497	
39	..	..	..	...	...	24,8	..	The pend. N° 2. ruined.

The bullets were now put naked into the piece, and the powder was lighted by the short vent-tube (*v*, fig. 3) and some little improvement was made in the steel edges between which the ribbons passed that served to measure the ascending arcs of the pendulum and of the recoil, by which means the friction was lessened, and the ribbon was prevented from twisting or entangling itself as it was drawn out.

Apparatus.

The barrel with its carriage as before.
The pendulum, N° 3. and
Leaden bullets, weighing 580 grains each.

Order of the experiments.	The charge of powder. Weight.	The charge of powder. Height.	Vent from the bottom of the charge.	Chord of the afcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
40	218	1,9	0	6,45	64,6	18,	1236	5th day; medium velocity in theſe experiments and N° 47. 1225.
41	..	..	..	6,31	65,3	17,71	1197	
42	..	..	..	6,45	65,	17,91	1230	
43	..	..	1,3	6,5	64,6	18,3	1248	Medium velocity 1276
44	..	..	..	6,75	64,5	18,35	1299	
45	..	..	..	6,6	64,9	...	1265	
46	..	..	..	6,4	61,6	...	1293	
47	..	..	0	6,3	62,	18,1	1266	
48	290	2,6	0	7,2	63,5	22,58	1414	Medium velocity 1427.
49	..	..	..	7,4	...	22,92	1455	
50	..	..	..	7,3	64,6	22,38	1412	
51	290	2,6	1,3	7,4	63,	23,21	1476	Medium velocity 1493.
52	..	..	..	7,6	64,	23,76	1520	
53	..	..	..	7,25	61,	23,6	1483	
54	..	..	2,6	7,5	62,3	...	1502	Medium velocity 1460.
55	..	..	..	7,4	64.	23,26	1450	
56	..	..	..	7,1	62,2	...	1433	
57	..	..	..	7,4	64,	23.56	1454	
58	..	..	..	1,31		11,12	—	In theſe 4 experiments the piece was fired with powder alone, and the ſcreen was taken away from before the pendulum.
59	..	..	..	1,2		11,62	—	
60	..	..	0	1,16		9,62	—	
61	..	..	1,3	0,6		11,33	—	

Order

Order of the experiments.	The charge of powder. Weight.	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N° 62	330	2,9	1,3	8,	63,	26,4	1599	6th day; medium velocity 1625.
63	..	..	..	8,5	65,	....	1652	
64	..	..	2,6	7,2	59,5	25,3	1562	Medium velocity 1528.
65	..	..	..	7,7	65,	...	1495	
66	..	..	0	8,4	...	26,35	1633	Medium velocity 1594.
67	..	..	..	8,	...	25,8	1556	
68	218	1,9	0	6,82	64,	19,56	1349	The powder was rammed very hard.
69	..	..	..	6,6	64,6	18,2	1294	Ditto much harder.
70	..	..	..	6,85	...	19,12	1345	Ditto as hard as in N° 68.
71	..	..	1,3	5,5	...	16,33	1080	Ditto, ditto,
72	..	..	0	——	——	8,72	—	Government powder, no bullet.
73	..	..	..	——	——	8,44	—	Best double battle powder.
74	..	..	1,3	——	——	8,47	—	Government powder.
75	..	..	..	——	——	9,3	—	Double battle powder.

The following experiments N° 78, 79, 80, and 81. were made in hopes of being able to discover a method of adding to the force of gun-powder. Twenty grains of the substances mentioned in the remarks upon each experiment were intimately mixed with the powder of the charge. In the experiment N° 82. a large wad of tow, well soaked in etherial spirit of turpentine, was put into the piece immediately upon the bullet: and in the experiment N° 83. a wad, soaked in alkohol, was put into the piece in like manner.

Order of the experiments.	The charge of powder. Weight.	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Ballet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the ballet.	Remarks.
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N° 76	145	1,3	0	5,3	65,	13,25	1037	7th day; medium velocity 1040.
77	..	..	..	...	64,6	13,25	1044	
78	..	..	..	3,2	...	8 92		20 grs. best alkaline salt of tartar.
79	..	..	..	4,35	...	11,68		20 grs. æthiops mineral.
80	..	..	..	3,3	62,6	9,83		20 grs. sal ammon.
81	..	..	..	4,2	63,4	11,45		20 grs. fine brass dust.
82	..	..	..	——	——	15,25	—	The screws which held the hooks by which the pendulum was suspended gave way, and the pendulum came down.
83	..	..	..	——	——	14,35	—	

In the nine following experiments, *viz.* from N° 84. to N° 92. inclusive, the valve-vent was made use of, and the bullets were made to fit the bore of the piece very exactly by means of oiled leather, which was so firmly fastened about them that in each experiment it entered the target with the bullet.

The bullet made use of in experiment N° 85. was of wood.

Those used in the experiments N° 86. and N° 87. were formed in the following manner; a small bullet was cast of plaister of Paris, which being thoroughly dried, and well heated at the fire, was fixed in the center of the mould that served for casting all the leaden bullets made use of in these experiments; and melted lead being poured into this mould, the cavity that surrounded the small plaister bullet was intirely filled up, and a bullet was produced, which to the eye had every appearance of solidity, but was as much lighter than a solid leaden bullet of the

the ſame diameter as the plaiſter bullet was lighter than a leaden bullet of the ſame ſize.

In the experiments N° 88. and N° 89. ſolid leaden bullets were made uſe of. In the experiment N° 90. two bullets were diſcharged at once; in the experiment N° 91. three; and in the experiment N° 92. four were uſed.

In each of theſe experiments a freſh ſheet of paper was made uſe of as a ſcreen to the pendulum, in order that the velocities of the bullets might be meaſured more accurately; and alſo, that the quantity of unfired powder might be eſtimated with greater preciſion.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Weight of the bullet.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.							
	Grs.	In.	In.	Grs.	Inches.	Inches.	Inches.	Ft. in ſec.	
N°									
84	145	1,3	0	—	—	—	4,5	—	8th day: in each of theſe 4 experiments from 50 to 70 granules or particles of unfired powder were driven through the ſcreen.
85	..	..	..	90	1,33	62,2	7,16	1763	
86	..	..	..	251	2,82	63,2	9,62	1317	
87	..	..	..	354	3,32	61,2	11,3	1136	
88	..	..	..	600	6,5	65,4	15,22	1229	Very few unfired grains of powder ſtruck the ſcreen.
89	..	..	..	603	6,3	64,6	15,13	1229	
90	..	..	..	1184	10,12	65,	21,92	978	There were no marks of any unfired powder having reached the ſcreen.
91	..	..	..	1754	13,65	63,4	27,18	916	
92	..	..	..	2352	16,55	63,3	32,25	833	

In

In the seven following experiments the piece was fired with powder only.

Order of the experiments.	The charge of powder. Wright	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Chord of the arc of the recoil.	Remarks.
	Grs.	In.	In.	Inches.	Inches.	
N°						
93	145	1,3	0	——	4,3	
94	165	1,45	..	——	5,5	
95	..	..	..	——	5,6	
96	290	2,6	..	——	11,70	
97	437½	3,9	..	1,68	17,5	The screen was taken away.
98	..	..	..	6,7	15,88	The whole surface of the target was besmeared with unfired grains of powder.
99	..	..	..	——	17,9	The pendulum was not observed.

In the following experiments N° 100. and N° 101. the bullets were not put down into the bore, but were supported by three wires, which being fastened to the end of the barrel projected beyond it, and confined the bullet in such a situation that its center was in a line with the axis of the bore, and its hinder part was one-twentieth of an inch without or beyond the mouth of the piece.

In experiment N° 102. the bullet was just stuck into the barrel in such a manner that near one-half of it was without the bore.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the afcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
N°								
100	165	1,45	0	,65	60,5	4,9	138	In each of theſe experim. near $\frac{1}{10}$th part of the ſubſtance of the bullet was melted and blown away by the impulſe of the flame.
101	..	..	..	,43	uncertain.	4,8	92	
102	..	..	..	,86	63,	5,6	180	

All that part of the bullet which lay towards the bore of the piece appeared to be quite flat from the loſs of ſubſtance it had ſuſtained; and its ſurface was full of ſmall indents, which probably were occaſioned by the unfired grains of powder that impinged againſt it.

The

The following experiments were made with the pendulum N° 4. The reſt of the apparatus as before.

Order of the experiments.	The charge of powder. Weight.	Height.	Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	In.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
								9th day.
N° 103	104	,9	0	4,51	65,	10,6	732	About 40 grains of powder were driven through the ſcreen.
104	145	1,3	..	5,4	...	12,92	877	About 40 unfired grs. of powder. Medium velocity 894.
105	..	..	..	5,6	...	13,28	910	40 unfired grains.
106	..	1,14	..	6,18	65,8	14,3	990	Double proof battle powder; no unfired grains.
107	218	1,8	..	8,48	65,	19,68	1380	Ditto, ditto.
108	290	2,6	..	9,45	65,6	23,9	1526	Government powder; bullet leathered; weight 602 grains.
109	..	..	..	8,73	65,2	22,8	1419	Bullet naked; very few unfired grains.
110	..	..	..	9,3	65,6	23,4	1460	Medium velocity 1444.
111	..	..	..	...	...	...	1462	
112	..	..	..	8,85	65,5	22,94	1436	
113	..	..	2,6	8,65	64,	23,7	1438	Medium velocity 1413.
114	..	..	..	8,5	63,6	24,1	1423	
115	..	..	..	8,4	65,	23,8	1378	
116	..	2,28	..	9,15	64,	24,6	1525	Double proof battle powder.
117	437½	3,9	..	10,56	64,9	33,	1738	Gov. pow. No unfired grains thro' the ſcreen.
118	..	..	..	11,	64,5	33,3	1824	Medium velocity 1764.
119	..	..	..	10,5	65,	33,6	1729	
120	..	..	2,6	10,35	...	32,5	1706	Medium velocity 1751.
121	..	..	..	10,65	...	33,2	1757	
122	..	..	..	10,6	63,6	32,9	1789	
123	..	..	0	——	——	17,9	—	Without any bullet.

Of the method made uſe of for computing the velocities of the bullets.

As the method of computing the velocity of a bullet from the arc of the vibration of a pendulum into which it is fired is ſo well known, I ſhall not enlarge upon it in this place, but ſhall juſt give the theorems that have been propoſed by different authors, and ſhall refer thoſe who wiſh to ſee more on the ſubject to Mr. ROBINS's New Principles of Gunnery; to Profeſſor EULER's Obſervations upon Mr. ROBINS's Book; and, laſtly, to Dr. HUTTON's Paper on the initial Velocities of Cannon Balls, which is publiſhed in the Tranſactions of the Society for the year 1778.

If a denote the length from the axis of the pendulum to the ribbon which meaſures the chord of the arc of its vibration;

g, the diſtance of the center of gravity below the axis;
f, the diſtance of the center of oſcillation;
b, the diſtance of the point ſtruck by the bullet;
c, the chord of the aſcending arc of the pendulum;
P, the weight of the pendulum;
b, the weight of the bullet, and
v, the original velocity of the bullet;

$v = \frac{c}{a} \times \sqrt{\frac{Pg}{bb}} + \frac{b}{f} \times \frac{f}{\sqrt{2b}}$, is a theorem for finding the velocity upon Mr. ROBINS's principles.

*$v = \frac{c}{a} \times \sqrt{\frac{Pg}{bb}} + \frac{f+b}{2f} \times \sqrt{\frac{f}{2}}$, is the theorem propoſed by Profeſſor EULER, who has corrected a ſmall error in Mr. ROBINS's method; and

* Put the rational part $\frac{c}{a} \times \sqrt{\frac{Pg}{bb}} + \frac{f+b}{2f} = n$, and expreſs f in the thouſandth parts of a Rhynland foot; then the velocity with which the ball ſtrikes the pendulum will be $= \frac{n}{4}\sqrt{\frac{f}{2}}$ Rhynland feet in a ſecond.

$v =$

$v = 5{,}672\, eg\sqrt{f \times \frac{P+b}{bba}}$ is Dr. HUTTON's theorem, which is ſufficiently accurate, and far more ſimple and expeditious than either of the preceding. It is to be remembered, that g, b, and c, may be expreſſed in any meaſure; but f muſt be Engliſh feet, and v will be the velocity of the bullet in Engliſh feet in a ſecond.

The velocities of the bullets in moſt of the foregoing experiments were firſt computed by EULER's method, as I had not then ſeen Dr. HUTTON's paper; but in going over the calculations a ſecond time, I made uſe of Dr. HUTTON's theorem. Both theſe methods gave the ſame velocity very nearly, but the Doctor's method is by much the eaſieſt in practice.

In theſe computations care was taken to make a proper allowance for the bullets that were lodged in the pendulum, and alſo for the velocity loſt by the bullet in paſſing through the ſcreen.

The corrections neceſſary on account of the bullets lodged in the pendulum were made in the following manner.

b was continually added to the value of P,

$\frac{b-g}{P} \times b$ . . . to the value of g, and

$\frac{f-b}{P} \times b$ . . . to the value of f.

Of the ſpaces occupied by the different charges of powder.

The heights of the charges of powder, or the lengths of the ſpaces which they occupied in the bore, were determined by meaſurement; and in order that this might be done with greater accuracy, inches and tenths of inches were marked upon the ram-rod, and the charge was gently forced down till it occupied the ſame ſpace in each experiment.

The following table ſhews the heights of the charges as they were determined by meaſurement, and alſo their heights computed

puted from the diameter of the bore of the piece, and the ſpecific gravity of the powder that was made uſe of.

N. B. By an experiment I ſhall give an account of hereafter, I found the ſpecific gravity of this powder ſhaken well together to be to that of rain water as 0,937 is to 1,000.

Weight of the powder.	Height of the charge.	
	Meaſured.	Computed.
Grs.	Inches.	Inches.
104	,9	0,8957
145	1,3	1,2490
165	1,45	1,4211
208	1,8	1,7914
218	1,9	1,8775
290	2,6	2,4980
310	2,7	2,6700
330	2,9	2,8422
416	3,6	3,5828
437½	3,9	3,7680

In the experiment N° 30. the powder was put into a cartridge ſo much ſmaller than the bore of the piece, that the charge, inſtead of occupying 1,45 inches, extended 3,2 inches. By this diſpoſition of the powder, its action upon the bullet appears to have been very much diminiſhed.

Of the effect that the heat which pieces acquire in firing produces upon the force of powder.

It is very probable, that the exceſs of the velocity of the bullet in the ſecond experiment over that of the firſt was occaſioned more by the heat the barrel had acquired in the firſt experiment than by the poſition of the vent, or any other circumſtance; for I have ſince found, upon repeated trials, that the force of any given charge of powder is conſiderably greater when it is fired in a piece that has been previouſly heated by firing, or by any other means, than when the piece has not been heated. Every body that is acquainted with artillery knows, that the recoil of great guns is much more violent after the ſecond or third diſcharge than it is at firſt; and on ſhip-board, where

where it is neceſſary to attend to the recoil of the guns, in order to prevent very dangerous accidents that might be occaſioned by it, the conſtant practice has been in our navy, and, I believe, on board the ſhips of all other nations, to leſſen the quantity of powder after the firſt four or five rounds: our 32 pounders, for inſtance, are commonly fired with 14 lbs. of powder at the beginning of an action, but the charge is very ſoon reduced to 11 lbs. and afterwards to 9 lbs. and the filled cartridges are prepared accordingly.

By the recoil it ſhould ſeem, that the powder exerted a greater force alſo in the fourth experiment, being the ſecond upon the ſecond day, than it did upon the third, or the firſt upon that day; but the pendulum giving way, it was not poſſible to compare the velocities of the bullets in the manner we did in the two experiments mentioned above.

This augmentation of the force of powder, when it is fired in a piece that is warm, may be accounted for in the following manner. There is no ſubſtance we are acquainted with that does not require to be heated before it will burn; even gun-powder is not inflammable when it is cold. Great numbers of ſparks or red-hot particles from the flint and ſteel are frequently ſeen to light upon the priming of a muſket, without ſetting fire to the powder, and grains of powder may be made to paſs through the flame of a candle without taking the fire; and what is ſtill more extraordinary, if large grains of powder are let fall from the height of two or three feet upon a red-hot plate of iron, laid at an angle of about 45° with the plane of the horizon, they will rebound intire without being burnt, or in the leaſt altered, by the experiment. In all theſe caſes the fire is too feeble, or the duration of its action is not ſufficiently

long,

long to heat the powder to that degree which is neceſſary in order to its being rendered inflammable.

Now as gun-powder, as well as all other bodies, acquires heat by degrees, and as ſome ſpace of time is taken up in this as well as in all other operations, it follows, that powder, which has been warmed by being put into a piece made hot by repeated firing, is much nearer that ſtate in which it will burn, or, I may ſay, is more inflammable than powder which is cold; conſequently, more of it will take fire in a given ſhort ſpace of time, and its action upon the bullet and upon the gun will of courſe be greater.

The heat of the piece will alſo ſerve to dry the air in the bore, and to clear the inſide of the gun of the moiſture that collects there when it has not been fired for ſome time, and theſe circumſtances doubtleſs contribute ſomething to the quickneſs of the inflammation of the powder, and conſequently to its force.

As it takes a longer time to heat a large body than a ſmall one, it follows, that meal-powder is more inflammable than that which is grained; and the ſmaller the particles are, the quicker they will take fire. The ſailors bruiſe the priming after they have put it to their guns, as they find it very difficult, without this precaution, to fire them off with a match: and if thoſe who are fond of ſporting would make uſe of a ſimilar artifice, and prime their pieces with meal-powder, they would miſs fire leſs often, the ſprings of the lock might be made more tender, and its ſize conſiderably reduced without any riſque, and the violence of the blow of the flint and ſteel in ſtriking fire being leſſened, the piece might be fired with greater preciſion.

Concluding from the result of the four experiments mentioned above, as well as from the reasons just cited, that the temperature of the piece has a considerable effect upon the force of the powder, I afterwards took care to bring the barrel to a proper degree of heat, by firing it once or oftener with powder each time I recommenced the experiments after the piece had been left to cool.

Of the manner in which pieces acquire heat in firing.

I was much surprised upon taking hold of the barrel immediately after the experiment N° 17. when it was fired with 330 grains of powder without any bullet, to find it so very hot that I could scarcely bear it in my hand, evidently much hotter than I had ever observed it before, notwithstanding the same charge of powder had been made use of in the two preceding experiments, and in both these experiments the piece was loaded with a bullet, which one would naturally imagine, by confining the flame, and prolonging the time of its action, would heat the barrel much more than when it was fired with powder alone.

I was convinced that I could not be mistaken in the fact, for it had been my constant practice to take hold of the piece to wipe it out as soon as an experiment was finished, and I never before had found any inconvenience from the heat in holding it. But in order to put the matter beyond all doubt, after letting the barrel cool down to the proper temperature, I repeated the experiment twice with the same charge of powder and a bullet; and in both these trials (experiments N° 18. and N° 19.) the heat of the piece was evidently much less than what it was in the experiment above mentioned (N° 17.).

I now regretted exceedingly the loss of a small pocket thermometer, which I had provided on purpose to measure the heat of the barrel, but it was accidentally broken by a fall the day before I began my experiments; and being so far from London, I had it not in my power to procure another: I was therefore obliged to content myself with determining the heat of the piece as well as I could by the touch.

Being much struck with this accidental discovery of the great degree of heat that pieces acquire when they are fired with powder without any bullet, and being desirous of finding out whether it is a circumstance that obtains universally, I was very attentive to the heat of the barrel after each of the succeeding experiments; and I constantly found the heat sensibly greater when the piece was fired with powder only, than when the same charge was made to impel one or more bullets.

Though the result of these experiments was totally unexpected, and even contrary to what I should have foretold if I had been asked an opinion upon the subject previous to making them; yet, after mature consideration, I am now convinced, that it is what ought to happen, and that it may be accounted for very well upon principles that are clearly admissible.

It is certain, that a very small part only of the heat that a piece of ordnance acquires in being fired is communicated to it by the flame of the powder; for the time of its action is so short (not being, perhaps, in general longer than about $\frac{1}{100}$th or $\frac{1}{110}$th part of a second) that if its heat, instead of being 4 times, as Mr. ROBINS supposes, was 400 times hotter than red-hot iron, it could not sensibly warm so large a body of metal as goes to form one of our large pieces of cannon. And besides, if the heat of the flame was sufficiently intense to produce so great an effect in so short a time, it would certainly be

ſufficient to burn up all inflammable bodies that it came near, and to melt the ſhot that it ſurrounded and impelled, eſpecially when they were ſmall, and were compoſed of lead or any other ſoft metal; but, on the contrary, we frequently ſee the fineſt paper come out of the mouth of a piece uninflamed, after it has ſuſtained the action of the fire through the whole length of the bore, and the ſmalleſt lead ſhot is diſcharged without being melted.

But it may be objected here, that bullets are always found to be very hot if they are taken up immediately after they come out of a gun; and that this circumſtance is a proof of the intenſity of the heat of the flame of powder, and of its great power of communicating heat to the denſeſt bodies. But to this I anſwer, I have always obſerved the ſame thing of bullets diſcharged from wind-guns and croſs-bows, eſpecially when they have impinged againſt any hard body, and are much flattened; and bullets from muſkets are always found to be hotter in proportion to the hardneſs of the body againſt which they are fired. If a muſket ball is fired into any very ſoft body, as (for inſtance) into water, it will not be found to be ſenſibly warmed; but if it is fired againſt a thick plate of iron, or any other body that it cannot penetrate, the bullet will be demoliſhed by the blow, and the pieces of it that are diſperſed about will be found to be in a ſtate very little ſhort of fuſion, as I have often found by experience. It is not by the flame therefore that bullets are heated, but by percuſſion. They may, indeed, receive ſome ſmall degree of warmth from the flame, and ſtill more perhaps by friction againſt the ſides of the bore, but it is in ſtriking againſt hard bodies, and from the reſiſtance they meet with in penetrating thoſe that are ſofter, that they acquire by far

the greater part of the heat we find in them as ſoon as they come to be at reſt, after having been diſcharged from a gun.

There is another circumſtance that may poſſibly be brought as an objection to this opinion, and that is the running of the metal in braſs guns upon repeatedly firing them, by which means the vent is often ſo far enlarged as to render the piece intirely uſeleſs. But this, I think, proves nothing but that braſs is very eaſily corroded, and deſtroyed by the flame of gun-powder; for it cannot be ſuppoſed, that in theſe caſes the metal is ever fairly melted. The vent of a muſket is very ſoon enlarged by firing, and after a long courſe of ſervice it is found neceſſary to ſtop it up with a ſolid ſcrew, through the center of which a new vent is made of the proper dimenſions. This operation is called buſhing, or rather bouching the piece; but in all the better kind of fowling-pieces the vent is lined, or bouched, with gold, and they are found to ſtand fire for any length of time without receiving the leaſt injury. But every body knows that gold will run with a leſs heat than is required to melt iron: but gold is not corroded either by the ſpirit of nitre, or the acid ſpirit that is generated from ſulphur, whereas iron is very eaſily deſtroyed by either; and that I take to be the only reaſon why a vent that is lined with gold is ſo much more durable than one that is made in iron. But it ſeems, that iron is more durable than braſs; and perhaps ſteel, or ſome other cheap metal, may be found that will ſupply the place of gold, and by that means the great expence that attends bouching pieces with that precious metal may be ſpared, and this improvement may be introduced into common uſe.

This leads us to a very eaſy and effectual remedy for that defect ſo long complained of in all kinds of braſs ordnance, *the running of the vent*; for if theſe pieces were bouched with iron,

iron, there is no doubt but they would ſtand fire as well as iron guns; and if ſteel, or any other metal, either ſimple or compounded, ſhould upon trial be found to anſwer for that purpoſe better than iron, it might be uſed inſtead of it; and even if gold was made uſe of for lining the vent, I imagine it might be done in ſuch a manner as that the expence would not be very conſiderable, at the ſame time that the thickneſs of the gold ſhould be ſufficient to withſtand the force of the flame for a very great length of time.

But to return to the heat acquired by guns in firing. It being pretty evident that it is not all communicated by the flame, there is but one other cauſe to which it can be attributed, and that is the motion and friction of the internal parts of the metal among themſelves, occaſioned by the ſudden and violent effort of the powder upon the inſide of the bore, and to this cauſe I imagine the heat is principally if not almoſt intirely owing. It is well known, that a very great degree of heat may be generated in any hard and denſe body in a ſhort ſpace of time by friction, and in a ſtill ſhorter time by colliſion. "For if two denſe hard elaſtic bodies be ſtruck againſt "each other with great force and velocity, all the parts of "ſuch bodies will every moment be cloſely compreſſed, and "being rigid will re-act with equal force. Hence a quick and "powerful contraction and expanſion will ariſe in every part, "reſembling that ſwift kind of vibrations obſerved in ſtretched "ſtrings; how great theſe vibrations are may be learnt from "the inſtance of a bell, when ſtruck with a ſingle blow, by "which the whole bulk, however vaſt, will for a long time "expand and contract itſelf in infinite ellipſes. And when "the attrition above deſcribed is produced, with what force and "velocity are all the particles of the rubbed body compreſſed,

" ſhaken, and looſened to their very intimate ſubſtance*?" And in proportion to the ſwiftneſs of this vibration, and the violence of the attrition and friction, will be the heat that is produced.

A piece of iron that would ſuſtain the preſſure of any weight, however large, without being warmed, may be made quite hot by the blow of a hammer; and even ſoft and un-elaſtic bodies may be warmed by percuſſion, provided the velocity with which their parts are made to give way to the blow is ſufficiently rapid. If a leaden bullet is laid upon an anvil, or any other hard body, and in that ſituation it is ſtruck with a ſmart blow of a hammer, it will be found to be much heated; but the ſame bullet in the ſame ſituation may be much more flattened by preſſure, or by the ſtroke of a very heavy body moving with a ſmall velocity, without being ſenſibly warmed.

To generate heat therefore the action of the powder upon the inſide of the piece muſt not only be ſufficient to ſtrain the metal, and produce a motion in its parts, but this effect muſt be extremely rapid; and the heat will be much augmented, if the exertion of the force and the duration of its action are momentaneous; for in that caſe, the fibres of the metal (if I may uſe the expreſſion) that are violently ſtretched, will return with their full force and velocity, and the ſwift vibratory motion and attrition before deſcribed will be produced.

The heat generated in a piece by firing is therefore as the force by which the particles of the metal are ſtrained and compreſſed, the ſuddenneſs with which this force is exerted, and the ſhortneſs of the time of its action; that is to ſay, as the ſtrength of the powder and the quantity of the charge, the quickneſs of its inflammation, and the velocity with which the generated fluid makes its eſcape.

* Vide SHAW's tranſlation of BOERHAAVE's Chemiſtry, vol. I. p. 229.

Now

Now the effort of any given charge of powder upon the gun is very nearly the ſame, whether it be fired with a bullet or without; but the velocity with which the generated elaſtic fluid makes its eſcape, is much greater when the powder is fired alone, than when it is made to impel one or more bullets; the heat ought therefore to be greater in the former caſe than in the latter, as I found by experiment.

But to make this matter ſtill plainer, we will ſuppoſe any given quantity of powder to be confined in a ſpace that is juſt capable of containing it, and that in this ſituation it is by any means ſet on fire. Let us ſuppoſe this ſpace to be the chamber of a piece of ordnance of any kind, and that a bullet, or any other ſolid body, is ſo firmly fixed in the bore immediately upon the charge, that the whole effort of the powder ſhall not be able to remove it. As the powder goes on to be inflamed, and the elaſtic fluid is generated, the preſſure upon the inſide of the chamber will be increaſed, till at length all the powder being burnt, the ſtrain upon the metal will be at its greateſt height, and in this ſituation things will remain, the coheſion or elaſticity of the particles of metal counterbalancing the preſſure of the fluid.

Under theſe circumſtances very little heat would be generated; for the continued effort of the elaſtic fluid would approach to the nature of the preſſure of a weight; and that concuſſion, vibration, and friction, among the particles of the metal, which in the colliſion of elaſtic bodies is the cauſe of the heat that is produced, would ſcarcely take effect.

But inſtead of being firmly fixed in its place, let the bullet now be moveable, but let it give way with great difficulty, and by ſlow degrees. In this caſe, the elaſtic fluid will be generated as before, and will exert its whole force upon the chamber

of

of the piece; but as the bullet gives way to the preſſure, and moves on in the bore, the fluid will expand itſelf and grow weaker, and the particles of the metal will gradually return to their former ſituations; but the velocity with which the metal reſtores itſelf being but ſmall, the vibration that remains in the metal, after the elaſtic fluid has made its eſcape, will be very languid, as will be the heat that is generated by it.

But if, inſtead of giving way with ſo much difficulty, the bullet is much lighter, ſo as to afford but little reſiſtance to the elaſtic fluid in making its eſcape, or if the powder is fired without any bullet at all; then, there being little or nothing to oppoſe the flame in its paſſage through the bore, it will expand itſelf with an amazing velocity, and its action upon the gun will ceaſe almoſt in an inſtant, the ſtrained metal will reſtore itſelf with a very rapid motion, and a ſharp vibration will enſue, by which the piece will be much heated.

Of the effect of ramming the powder in the chamber of the piece.

The charge, conſiſting of 218 grains of powder, being put gently into the bore of the piece in a cartridge of very fine paper, without being rammed, the velocity of the bullets at a mean of the 40th, 41ſt, 42d, and 47th experiments, was at the rate of 1225 feet in a ſecond; but in the 68th, 69th, and 70th experiments, when the ſame quantity of powder was rammed down with five or ſix hard ſtrokes of the ram-rod, the mean velocity was 1329 feet in a ſecond. Now the total force or preſſure exerted by the charge upon the bullet is as the ſquare of its velocity, and $\overline{1329}^2$ is to $\overline{1225}^2$ as 1,1776 is to 1; or nearly

nearly as 6 is to 5; and in that proportion was the force of the given charge of powder increaſed by being rammed.

In the 71ſt experiment the powder was alſo rammed, but the vent, inſtead of being at the bottom of the bore, was at 1,3, and the velocity of the bullet was very conſiderably diminiſhed, being only at the rate of 1080 feet in a ſecond, inſtead of 1276 feet in a ſecond, which was the mean velocity with this charge, and with the vent in this ſituation when the powder was rammed. See the experiments N° 43, 44, 45, and 46.

When, inſtead of ramming the powder, or preſſing it gently together in the bore, it is put into a ſpace larger than it is capable of filling, the force of the charge is thereby very ſenſibly leſſened, as Mr. ROBINS and others have found by repeated trials. In my 30th experiment the charge, conſiſting of no more than 165 grains of powder, was made to occupy 3,2 inches of the bore inſtead of 1,45 inches, which ſpace it juſt filled when it was gently puſhed into its place without being rammed; the conſequence was, the velocity of the bullet, inſtead of being 1100 feet in a ſecond or upwards, was only at the rate of 914 feet in a ſecond, and the recoil was leſſened in proportion.

And from hence we may draw this practical inference, that the powder, with which a piece of ordnance or a fire-arm is charged, ought always to be preſſed together in the bore; and if it is rammed to a certain degree, the velocity of the bullet will be ſtill farther increaſed. It is well known, that the recoil of a muſket is greater when its charge is rammed than when it is not; and there cannot be a ſtronger proof that ramming increaſes the force of the powder.

Of the relation of the velocities of bullets to the charges of powder by which they are impelled.

It appears by all the experiments that have hitherto been made upon the initial velocities of bullets, that when the weights and dimenſions of the bullets are the ſame, and they are diſcharged from the ſame piece by different quantities of powder, the velocities are in the ſub-duplicate ratio of the weights of the charges very nearly.

The following table will ſhew how accurately this law obtained in the foregoing experiments.

Charges.	Velocities		Difference.	N° of exp.
	Computed.	Actual.		
437½	1764	1764	——	3
330	1533	1594	+ 61	2
310	1486	1459	− 27	1
290	1436	1436	0	7
218	1232	1225	− 7	4
208	1216	1256	+ 40	3
165	1083	1087	+ 4	2
145	1018	1040	+ 22	2
104	860	757	− 103	2

The computed velocities, as they are ſet down in this table, were determined from the ratio of the ſquare root of 437½ (the weight in grains of the largeſt charge of powder) to the mean velocity of the bullet with that charge and the vent at 0; *viz.* 1764 feet in a ſecond, and the ſquare root of the other charges expreſſed in grains. And the *actual* velocities are means of all experiments that were made under ſimilar circumſtances.

The fourth column ſhews the difference of the computed and actual velocities, or the number of feet in a ſecond by which the actual velocity exceeds or falls ſhort of the computed: and in the fifth column is ſet down the number of experiments with each charge, from the mean of which the actual velocity was determined.

The agreement of the computed and actual velocities will appear more ſtriking, if we take the ſum and difference of thoſe velocities with all the charges except the firſt: thus,

Sum of the velocities, -1764.			
Computed.	Actual.	Difference.	N° of exp.
9864	9854	-10	23

So that it appears, that the difference, or the actual velocity, was ſmaller than the computed by $\frac{1}{211}$ part only at a mean of 23 experiments.

But as by far the greater number of the experiments were made with the following charges, *viz.* 290, 218, 208, 165, and 145 grains of powder, let us take the ſum and difference of the computed and actual velocities of thoſe charges: thus,

Sum of the velocities			
Computed.	Actual.	Difference.	N° of exp.
5985	6044	$+59$	18

Here the agreement of the theory with the experiments is ſo very remarkable, that we muſt ſuppoſe it was in ſome meaſure accidental; for the difference of the velocities in repeating the ſame experiment is in general much greater than the difference of the computed and actual velocities in this inſtance; but, I think, we may fairly conclude, from the reſult of all theſe

trials, that the velocities of like *muſket* bullets, when they are diſcharged from the ſame piece by different quantities of the ſame kind of powder, are very nearly in the ſub-duplicate ratio of the weights of the charges. Whether this law will hold good when applied to cannon balls, and bomb ſhells of large dimenſions, I dare not at preſent take upon me to decide; but, for ſeveral reaſons that might be mentioned, I am rather of opinion, that it will not; at leaſt not with that degree of accuracy which obtained in theſe experiments.

Of the effect of placing the vent in different parts of the charge.

There have been two opinions with reſpect to the manner in which gun-powder takes fire. Mr. ROBINS ſuppoſes that the progreſs of its inflammation is ſo extremely rapid, "that all "the powder of the charge is fired and converted into an elaſtic "fluid, before the bullet is ſenſibly moved from its place;" while others have been of opinion, that the progreſs of the inflammation is much ſlower, and that the charge is ſeldom or never completely inflamed before the bullet is out of the gun.

The large quantities of powder that are frequently blown out of fire arms un-inflamed, ſeem to favour the opinion of the advocates for the gradual firing; but Mr. ROBINS endeavours to account for that circumſtance upon different principles, and ſupports his opinion by ſhewing that every increaſe of the charge within the limits of practice produces a proportional increaſe of the velocity of the bullet, and that when the powder is confined by a great additional weight, by firing two or more bullets at a time inſtead of one, the velocity is not ſenſibly greater than it ought to be according to his theory.

If this were a queſtion merely ſpeculative, it might not be worth while to ſpend much time in the diſcuſſion of it; but as it is a matter upon the knowledge of which depends the determination of many important points reſpecting artillery, and from which many uſeful improvements may be derived, too much pains cannot be taken to come at the truth. Till the manner in which powder takes fire, and the velocity with which the inflammation is propagated, are known, nothing can with certainty be determined with reſpect to the beſt form for the chambers of pieces of ordnance, or the moſt advantageous ſituation for the vent; nor can the force of powder, or the ſtrength that is required in different parts of the gun, be aſcertained with any degree of preciſion.

As it would be eaſy to determine the beſt ſituation for the vent from the velocity of the inflammation of powder being known, ſo on the other hand I had hopes of being able to come at that velocity by determining the effect of placing the vent in different parts of the charge; for which purpoſe the following experiments were made.

A table

A table of experiments, shewing the effect of placing the vent in different parts of the charge.

Weight of the charge of powder.	Space occupied by the powder.	Vent from the bottom of the bore.	Velocity of the bullet at a medium.	Recoil measured upon the ribbon at a medium.	Number of experiments.
Grains.	Inches.	Inches.	Ft. in a sec.	Inches.	
165	1,45	0	1087	14,465	2
. . .	. . .	1,32	1082	14,31	3
218	1,9	0	1225	17,93	4
. . .	. . .	1,3	1276	18,34	4
290	2,6	0	1427	22,026	3
. . .	. . .	1,3	1493	23,34	3
. . .	. . .	2,6	1460	23,286	4
. . .	. . .	0	1444	23,135	4
. . .	. . .	2,6	1413	24,5	3
310	2,7	0	——	24,69	1
. . .	. . .	1,32	——	24,95	1
. . .	. . .	2,65	——	24,9	1
330	2,9	0	1594	26,075	2
. . .	. . .	1,3	1625	26,4	2
. . .	. . .	2,6	1525	25,3	2
437½	3,9	0	1764	33,3	3
. . .	. . .	2,6	1751	32,866	3

By the foregoing experiments it appears, first, that the difference in the force of any given charge of powder which arises from the particular situation of the vent is extremely small.

With 165 grains of powder, and the vent at 0, the velocity of the bullet at a mean of two experiments (*viz.* the 20th and 21st) was 1087 feet in a second; and with the same charge, and the vent at 1,32 inches, the velocity at a mean of the 22d, 23d, and 24th experiments, was 1082 feet in a second; the

the difference, equal five feet in a second, is less than what occurred in a repetition of the same experiment.

With 218 grains of powder, and the vent at 0, the velocity at a mean in the 40th, 41st, 42d, and 47th experiments, was at the rate of 1225 feet in a second; and with the same charge, and the vent at 1,3, the velocity was 1276 feet in a second at a mean of four experiments, *viz.* the 43d, 44th, 45th, and 46th.

In the first set of experiments, with 290 grains of powder, the velocities were,

Vent at 0.	Vent at 1,3.	Vent at 2,6.
1414	1476	1502
1455	1520	1450
1412	1483	1433
		1454
3)4281	3)4479	4)5839
Means 1427	1493	1460

See the experiments from N° 48. to N° 57. inclusive.

In the second set the velocities were,

Vent at 0.	Vent at 2,6.
1419	1438
1460	1423
1462	1378
1436	
4)5777	3)4239
Means 1444	1413

See the experiments from N° 109. to N° 115. inclusive.

And

And taking the means of all the velocities in both sets in each position of the vent it will be,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
Mean velocity	1436	1493	1437

The mean recoils in these experiments were,

Vent at 0.	Vent at 1,3.	Vent at 2,6.
22,88	23,34	23,61

In the experiments with 310 grains of powder the velocities of the bullets were not determined with sufficient accuracy to be depended on; but the recoils, which were measured with great nicety, were as follows, *viz.*

Vent at 0.	Vent at 1,3.	Vent at 2,6
24,69	24,95	24,9

With 330 grains of powder the mean velocities and recoils were,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
Velocities	1594	1625	1525
Recoils	26,075	26,4	25,3

In the experiments with 437½ grains (an ounce avoirdupois) of powder the velocities and recoils were,

Vent at 0.		Vent at 2,6.	
Velocity.	Recoil.	Velocity.	Recoil.
1738	33,	1707	32,5
1824	33,3	1757	33,2
1728	33,6	1789	32,9
3)5291	3)99,9	3)5253	3)98,6
Means 1764	33,3	1751	32,866

Secondly,

Secondly, From the result of all these experiments it appears, that the effect of placing the vent in different positions with respect to the bottom of the chamber is different, in different charges; thus, with 165 grains of powder the velocity of the bullet was rather diminished by removing the vent from 0, or the bottom of the bore to 1,32; but with 218 grains of powder the velocity was a little increased, as was also the recoil. With 290 grains of powder the velocity was greatest when the powder was lighted at the vent 1,3 which was near the middle of the charge, and rather greater when it was lighted at the top, or immediately behind the bullet, than when it was lighted at the bottom. And by the recoil it would seem, that the velocities of the bullets varied nearly in the same manner when the charge consisted of 310 grains of powder.

With 330 grains of powder, both the velocity and the recoil were greater when the powder was lighted at the middle of the charge, than when it was lighted at the bottom; but they were least of all when it was lighted near the top. And when *an ounce* of powder was made use of for the charge, its force was greatest when it was lighted at the bottom. But the difference in the force exerted by the powder which arose from the particular position of the vent was in all cases so inconsiderable (being, as I have before observed, less than what frequently occurred in repeating the same experiment) that no conclusion can be drawn from the experiments, except only this, that any given charge of powder exerts nearly the same force, whatever is the position of the vent.

And hence the following practical inference naturally occurs, *viz.* that in the formation of fire-arms no regard need be had to any supposed advantages that gun-smiths and others have hitherto imagined were to be derived from particular situations for

the vent, ſuch as diminiſhing the recoil, increaſing the force of the charge, &c.; but the vent may be indifferently in any part of the chamber where it will beſt anſwer upon other accounts: and there is little doubt but the ſame thing will hold good in great guns, and all kinds of heavy artillery.

Almoſt every workman who is at all curious in fire-arms has a particular fancy with regard to the beſt form for the bottom of the chamber, and the proper poſition of the vent. They in general agree, that the vent ſhould be as low or far back as poſſible, in order, as they pretend, to leſſen the recoil; but no two of them make it exactly in the ſame manner. Some make the bottom of the chamber flat, and bring the vent out even with the end of the breech-pin. Others make the vent ſlanting through the breech-pin, in ſuch a manner as to enter the bore juſt in its axis. Others again make the bottom of the chamber conical; and there are thoſe who make a little cylindric cavity in the breech-pin, of about two-tenths of an inch in diameter, and near half an inch in length, coinciding with the axis of the bore, and bring out the vent even with the bottom of this little cavity.

The objection to the firſt method is, the vent is apt to be ſtopped up by the foul matter that adheres to the piece after firing, and which is apt to accumulate, eſpecially in damp weather. The ſame inconvenience in a ſtill greater degree attends the other methods, with the addition of another, ariſing from the increaſed length of the vent; for the vent being longer it is not only more liable to be obſtructed, but it takes a longer time for the flame to paſs through it into the chamber, in conſequence of which the piece is ſlower in going off, or, as ſportſmen term it, is apt to hang fire.

The form I would recommend for the bottom of the bore is that of a hemiſphere; and the vent ſhould be brought out directly through the ſide of the barrel, in a line perpendicular to its axis, and pointing to the center of the hemiſpheric concavity of the chamber.

In this caſe the vent would be the ſhorteſt poſſible; it would be the leaſt liable to be obſtructed, and the piece would be more eaſily cleaned, than if the bottom of the bore was of any other form. All theſe advantages, and ſeveral others not leſs important, would be gained by making the bottom of the bore and vent of great guns in the ſame manner.

A new method of determining the velocities of bullets.

From the equality of *action* and *re-action* it appears, that the *momentum* of a gun muſt be preciſely equal to the momentum of its charge; or that the weight of the gun, multiplied into the velocity of its recoil, is juſt equal to the weight of the bullet and of the powder (or the elaſtic fluid that is generated from it) multiplied into their reſpective velocities: for every particle of matter, whether ſolid or fluid, that iſſues out of the mouth of a piece, muſt be impelled by the action of ſome power, which power muſt *re-act* with equal force againſt the bottom of the bore.

Even the fine inviſible elaſtic fluid that is generated from the powder in its inflammation cannot put itſelf in motion without re-acting againſt the gun at the ſame time. Thus we ſee pieces, when they are fired with powder alone, recoil as well as when their charges are made to impel a weight of ſhot, though the recoil is not in the ſame degree in both caſes.

It is eaſy to determine the velocity of the recoil in any given caſe, by ſuſpending the gun in an horizontal poſition by two pendulous rods, and meaſuring the arc of its aſcent, by means of a ribbon according to the method already deſcribed, and this will give the momentum of the gun, its weight being known, and conſequently the momentum of its charge. But in order to determine the velocity of the bullet from the recoil, it will be neceſſary to find out how much the weight and velocity of the elaſtic fluid contributes to it.

That part of the recoil which ariſes from the expanſion of this fluid is always very nearly the ſame, whether the powder is fired alone, or whether the charge is made to impel one or more bullets, as I have found by a great variety of experiments.

If therefore a gun, ſuſpended according to the method preſcribed, is fired with any given charge of powder, but without any bullet or wad, and the recoil is obſerved, and if the ſame piece is afterwards fired with the ſame quantity of powder, and a bullet of a known weight, the exceſs of the velocity of the recoil in the latter caſe, over that in the former, will be proportional to the velocity of the bullet; for the difference of theſe velocities, multiplied into the weight of the gun, will be equal to the weight of the bullet multiplied into its velocity.

Thus if W is put equal to the weight of the gun,

U = the velocity of its recoil, when it is fired with any given charge of powder, without any bullet,

V = the velocity of the recoil, when the ſame charge is made to impel a bullet,

B = the weight of the bullet, and

v = its velocity,

It will be $v = \frac{V - U \times W}{B}$.

Let us ſee how this method of determining the velocities of bullets will anſwer in practice.

In the 94th experiment the recoil, with 165 grains of powder, without a bullet, was 5,5 inches, and in the 95th experiment, with the ſame charge, the recoil was 5,6 inches. The mean is 5,55 inches; and the length of the rods by which the barrel was ſuſpended being 64 inches, the velocity of the recoil (= U) anſwering to 5,55 inches meaſured upon the ribbon, is that of 1,1358 feet in a ſecond.

In five experiments, with the ſame charge of powder, and a bullet weighing 580 grains, the recoil was as follows, *viz.*

The 20th experiment		14,73 inches
21ſt	-	14,2
22d	-	14,8
23d	-	14,58
24th	-	14,68
		5)73, (= 14,6 inches at a mean.

And the velocity of the recoil (= V) anſwering to the length is that of 2,9880 feet in a ſecond: conſequently V − U, or 2,9880 − 1,1358 is equal to 1,8522 feet in a ſecond.

But as the velocities of recoil are known to be as the chords of the arcs through which the barrel aſcends, it is not neceſſary in order to determine the velocity of the bullet to compute the velocities V and U; but the quantity $\overline{V-U}$, or the difference of the velocities of the recoil when the given charge is fired with and without a bullet, may be computed from the value of the difference of the chords, by one operation. Thus the velocity anſwering to the chord 9,05 is that of 1,8522 feet in a ſecond, which is juſt equal to $\overline{V-U}$, as was before found.

The

The weight of the barrel, together with its carriage, was 47¼ pounds, to which three quarters of a pound is to be added on account of the weight of the rods by which it was ſuſpended, which makes W = 48 pounds, or 336,000 grains, and the weight of the bullet was 580 grains. B is therefore to W as 580 is to 336,000, that is, as 1 is to 579,31 very nearly; and v $(= \frac{\overline{V-U} \times W}{B}$ is equal to $\overline{V-U} \times 579{,}31$.

The value of $\overline{V-U}$ anſwering to the experiments before mentioned was found to be 1,8522; conſequently the velocity of the bullets $(= v)$ was $1{,}8522 \times 579{,}31 = 1073$ feet in a ſecond, which is extremely near 1083 feet in a ſecond, the mean of the velocities, as they were determined by the pendulum.

But the computation for determining the velocity of a bullet upon theſe principles may be rendered ſtill more ſimple and eaſy in practice; for the velocities of the recoil being as the chords meaſured upon the ribbon, if

c is put equal to the chord of the recoil expreſſed in Engliſh inches, when the piece is fired with powder only, and

C = the chord when a bullet is diſcharged by the ſame charge,

then $C - c$ will be as $V - U$; and conſequently as $\frac{\overline{V-U} \times W}{B}$, which meaſures the velocity of the bullet, the ratio of W to B remaining the ſame.

If therefore we ſuppoſe a caſe in which $C - c$ is equal to one inch, and the velocity of the bullet is computed from that chord, the velocity in any other caſe, wherein $C - c$ is greater or leſs than one inch, will be found by multiplying the difference of the chords C and c by the velocity that anſwers to a difference of one inch.

The

The length of the parallel rods by which the barrel was ſuſpended being 64 inches, the velocity of the recoil anſwering to $C - c = 1$ inch meaſured upon the ribbon is 0,204655 parts of a foot in a ſecond; and this is alſo, in this caſe the value of $V - U$; the velocity of the bullet is therefore $v = 0{,}204655 \times 579{,}31 = 118{,}35$ feet in a ſecond.

Conſequently the velocity of the bullet expreſſed in feet *per* ſecond may in all caſes be found by multiplying the difference of the chords C, and c, by 118,35, the weight of the barrel, the length of the rods by which it is ſuſpended, and the weight of the bullet remaining the ſame, and this whatever the charge of powder may be that is made uſe of, and however it may differ in ſtrength or goodneſs.

According to this rule the velocities of the bullets in the following experiments have been computed from the recoil; and by comparing them with the velocities ſhewn by the pendulum, we ſhall be enabled to judge of the accuracy of this new method of determining the velocities of bullets.

In the 76th and 77th experiments, when the piece was fired with 145 grains of powder and a bullet, the recoil was 13,25 and 13,15, or 13,2 at a mean; and with the ſame charge of powder, without a bullet, the recoil was 4,5 and 4,3, or 4,4 at a mean (ſee the 84th and 93d experiments). $C - c$ is therefore $13{,}2 - 4{,}4 = 8{,}8$ inches, and the velocity of the bullets $= 8{,}8 \times 118{,}35 = 1045$ feet in a ſecond. The mean of the velocities as they were determined by the pendulum is that of 1040 feet in a ſecond. In the 104th and 105th experiments the recoil was 12,92 and 13,28, and the velocity computed from the mean of thoſe chords is 1030 feet in a ſecond; but the velocity ſhewn by the pendulum was no more than about 900 feet in a ſecond. As the recoil was ſo nearly equal to what

what it was in the 76th and 77th experiments before mentiond, when the velocities ſhewn by the recoil and by the pendulum were almoſt exactly the ſame, I am inclined to believe, that there muſt have been ſome miſtake in determining the velocities by the pendulum in theſe laſt experiments, and that the velocity ſhewn by the recoil is moſt to be depended on.

With 290 grains, or half the weight of the bullet in powder, in the 48th, 49th, and 50th experiments, the recoil was 22,58, 22,92, and 22,38; and the recoil, with the ſame charge of powder, without a bullet, at a mean of the 60th and 99th experiments, was 10,66. The mean of the velocities of the bullets, computed from the recoil, is therefore 1416 feet in a ſecond, and the velocity ſhewn by the pendulum was 1427 feet in a ſecond: the difference is not conſiderable. The mean of the velocities in the 109th, 110th, 111th, and 112th experiments is by the recoil 1464, and by the pendulum 1444 feet in a ſecond.

With 330 grains of powder the velocities of the bullets appear to have been as follows, *viz.*

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
By the recoil	1543	1620	1610
By the pendulum	1594	1625	1528

See the 62d, 63d, 64th, 65th, 66th, 67th, and 17th experiments.

The uniformity of the recoil was in all caſes very remarkable. Thus, in the firſt ſet of experiments with 290 grains of powder (from the 48th to the 57th experiment incluſive), the recoil was,

Vent

Vent at 0.	Vent at 1,3.	Vent at 2,6.
22,58	23,21	23,06
22,92	23,76	23,26
22,38	23,06	23,26
		23,56
3)67,88	3)70,03	4)93,14
Means = 22,626	23,343 and	23,285

If now we take a mean of the 60th and 99th experiments, and call the recoil, without a bullet, 10,66 as before, the velocities will turn out,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
By the recoil - -	1416	1501	1494
And by the pendulum they were	1427	1493	1494
The difference is only .	−11	+ 8	+34

The recoil was equally regular in the 117th and five ſucceeding experiments, when the charge was no leſs than 437½ grs. = 1 ounce avoirdupois in powder; and the velocities of the bullets determined from the recoil are very nearly the ſame as they were ſhewn by the pendulum. Thus, in the 117th, 118th, and 119th experiments the mean recoil was 33,3; and in the 120th, 121ſt and 122d experiments it was 32,866. And if the recoil without a bullet is called 17,9, as it was determined by the 123d experiment, which was made immediately after the experiments before mentioned, then will the velocities be,

	Vent at 0.	Vent at 2,6
By the recoil - -	1822	1771
And by the pendulum they were	1764	1751
The difference is only -	+58 and	+20 feet

in a ſecond, which is leſs than what frequently occurs in repeating the ſame experiment.

In the 11th, 12th, 13th, and 14th experiments, when the piece was fired with 310 grains of powder and a bullet, the recoil was 24,69, 24,95, 24,9, and 24,9: and in the 15th, 16th, 18th, and 19th experimentss with 330 grains of powder, the recoil was 26,2, 26,2, 26,3, and 26,4. The regularity of theſe numbers is very ſtriking; and though we cannot compare the velocities of the bullets determined by the two methods as we have done in other caſes (as there are reaſons to believe, that the velocities, as they are ſet down in the tables, are not much to be depended on, and as the recoil, with the given charge of 310 grains of powder without a bullet, is not known) yet the regularity of the recoil in theſe experiments affords good grounds to conclude, that the method of determining the velocities of bullets founded upon it muſt be very accurate.

But of all the experiments thoſe numbered from 84 to 92 incluſive afford the ſtrongeſt proof of the accuracy of this method. In thoſe every poſſible precaution was taken to prevent errors ariſing from adventitious circumſtances, and the weights of the bullets and their velocities were ſo various, that the uniform agreement of the two methods of determining the velocities in thoſe trials amounts almoſt to a demonſtration of the truth of the principles upon which this new method is founded.

By

By the following table the result of these experiments may be seen at one view.

The experiments.	Weight of the bullets.	The barrel heavier than the bullet.	The recoil.	Velocity of the bullet. By the recoil.	Velocity of the bullet. By the pendulum.	Difference.
	Grs.	$\frac{W}{B}=$	$C=$	$v=$	$v=$	
84th and 93d	——	—— $c=$	4,4	——	——	——
85th	90	3733,3	7,16	2109	1763	+346
86th	251	1338,6	9,62	1430	1317	+113
87th	354	949,15	11,03	1288	1136	+152
88th	600	560,	15,22	1240	1229	+ 11
89th	603	557,22	15,13	1224	1229	+ 5
90th	1184	283,78	21,92	1017	978	− 39
91ſt	1754	191,56	27,18	893	916	− 23
92d	2352	141,86	32,25	812	833	− 21

The charge of powder conſiſted of 145 grains in weight in each experiment.

In order to ſhew, in a more ſtriking manner, the reſult of theſe experiments, and the compariſon of the two methods of aſcertaining the velocities of bullets, I have drawn the fig. 16. where the numbers that are marked upon the line AB are taken from A towards B, in proportion to the weights of the bullets; while the lines drawn from thoſe numbers perpendicular from AB (as *w*, *v*, for inſtance, at the number 2352) and ending at the curve *c*, *d*, expreſs their velocities, as ſhewn by the pendulum. The continuation of thoſe lines on the oppoſite ſide of the line AB ſhew the recoil, and alſo the velocities of the bullets as determined from it; thus *w*, *r*, and the (dotted) lines

parallel to it, which end at the line *g*, *f*, expreſs the recoil; and the portion of each of thoſe lines that is comprehended between the line AB and the curve *n*, *n* (as *w*, *u*) is as the velocity of the bullet in the ſeveral experiments. The line A, *e*, denotes the weight of the charge of powder; and the line A, *m*, the velocity with which the claſtic fluid eſcapes out of the piece, when the powder is fired without any bullet.

Upon an inſpection of this figure, as well as from an examination of the foregoing table, it appears, that the velocities determined by the two methods agree with great nicety in all the experiments after the 87th; but in the 87th experiment, and alſo in the 86th, but particularly in the 85th, the difference in the reſult of theſe different methods is very conſiderable: and it is remarkable, that in thoſe experiments where they diſagree moſt, the velocities of the bullets, as determined by the pendulum, are extremely irregular; while, on the other hand, the gradual increaſe of the recoil as the bullets were heavier, and the great regularity of the correſponding velocities, afford good grounds to conclude, that this diſagreement is not owing to any inaccuracy in the new method of aſcertaining the velocities, but to ſome other cauſe that remains to be inveſtigated.

But before we proceed in this inquiry, let us ſeparate the five laſt experiments in the foregoing table; and, ſumming up the velocities determined by the two methods, we ſhall ſee by their difference how thoſe methods agreed upon the whole, in this inſtance.

Expe-

Experiments.	Weight of the bullets.	Velocity. By the recoil.	Velocity. By the pendulum.	Difference.
	Grs.			
88th	600	1240	1229	+11
89th	603	1224	1229	− 5
90th	1184	1017	978	+39
91ſt	1754	893	916	−23
92d	2352	812	833	−21
Sums and diff. of the velocities		5186	5185	+ 1

Here the difference in the reſult of the two methods does not amount to $\frac{1}{1000}$th part of the whole velocity; but I lay no ſtreſs upon this extraordinary argument. I am ſenſible that it muſt in ſome degree have been accidental; but as the difference in the velocities, computed by theſe different methods, was in no inſtance conſiderable, not being in any caſe ſo great as what frequently occurred in the moſt careful repetition of the ſame experiment, and as the velocities, as determined by the recoil, were much more regular than thoſe ſhewn by the pendulum, as appears by comparing the curves *g*, *f*, and *m*, *n*, (fig. 16.) with the crooked line *c*, *d*, I think we may fairly conclude, that this new method may with ſafety be relied on in practice.

The greateſt difference in the velocities, as aſcertained by the two methods, appears, in the inſtance of the 85th experiment, where the velocity determined from the recoil exceeds that ſhewn by the pendulum by 346 feet in a ſecond, the former velocity being that of 2109 feet in a ſecond, the latter only 1763 feet in a ſecond; and in the two ſucceeding experiments, the velocities ſhewn by the pendulum are likewiſe deficient, though not in ſo great a degree.

This

This apparent deficiency remains now to be accounted for; and, firſt, it cannot be ſuppoſed, that it aroſe from any imperfection in Mr. ROBINS's method of determining the velocities of bullets; for that method is founded upon ſuch principles as leave no room to doubt of its accuracy; and the practical errors that occur in making the experiments, and which cannot be intirely prevented, or exactly compenſated, are in general ſo ſmall, that the difference of the velocities in queſtion cannot be attributed to them. It is true, the effect of thoſe errors is more likely to appear in experiments made under ſuch circumſtances as thoſe under which the experiments we are now ſpeaking of were made, than in any other caſe; for the bullets being very light, the arc of the aſcent of the pendulum was but ſmall, and a ſmall miſtake in meaſuring the chord upon the ribbon would have produced a very conſiderable error in computing the velocity of the bullet: thus, a difference of one tenth of an inch, more or leſs, upon the ribbon in the 85th experiment, would have made a difference in the velocity of more than 120 feet in a ſecond. But independent of the pains that were taken to prevent miſtakes, the ſtriking agreement of the velocities determined by the two methods in the experiments which immediately follow, as alſo in all other caſes where they could be compared, affords abundant reaſon to conclude, that the errors ariſing from thoſe cauſes were in no inſtance very conſiderable.

But if both methods of aſcertaining the velocities of bullets are to be relied on, then the difference of the velocities, as determined by them in theſe experiments, can only be accounted for by ſuppoſing that it aroſe from their having been diminiſhed by the reſiſtance of the air in the paſſage of the bullets from the mouth of the piece to the pendulum; and this ſuſpicion will

will be much ſtrengthened when we conſider how great the reſiſtance is that the air oppoſes to bodies that move very ſwiftly in it, and that the bullets in theſe experiments were not only projected with great velocities, but were alſo very light, and conſequently more liable to be retarded by the reſiſtance on that account.

To put the matter beyond all doubt, let us ſee what the reſiſtance was that theſe bullets met with, and how much their velocities were diminiſhed by it. The weight of the bullet (in the 85th experiment) was 90 grains; its diameter was 0,78 of an inch, and it was projected with a velocity of 2109 feet in a ſecond.

If now a computation be made according to the method laid down by Sir ISAAC NEWTON for compreſſed fluids, it will be found, that the reſiſtance to this bullet was not leſs than 8½ lbs. avoirdupois, which is ſomething more than 660 times its weight. But Mr. ROBINS has ſhewn, by experiment, that the reſiſtance of the air to bodies moving in it with very great velocity is near three times greater than Sir ISAAC has determined it, and as the velocity with which this bullet was impelled is conſiderably greater than any in Mr. ROBINS's experiments, it is highly probable, that the reſiſtance in this inſtance was at leaſt 2000 times greater than the weight of the bullet.

The diſtance from the mouth of the piece to the pendulum, as we have before obſerved, was 12 feet; but, as there is reaſon to think, that the blaſt of the powder, which always follows the bullet, continues to act upon it for ſome ſenſible ſpace of time after it is out of the bore, and by urging it on counter-balances, or at leaſt counter-acts in a great meaſure the reſiſtance of the air, we will ſuppoſe, that the reſiſtance does

not begin, or rather that the motion of the bullet does not begin to be retarded, till it has got to the diſtance of two feet from the muzzle. The diſtance, therefore, between the barrel and the pendulum, inſtead of 12 feet, is to be eſtimated at 10 feet; and as the bullet took up about $\frac{1}{1000}$ part of a ſecond in running over that ſpace, it muſt in that time have loſt a velocity of about 335 feet in a ſecond, as will appear upon making the computation, and this will very exactly account for the apparent diminution of the velocity in the experiment; for the difference of the velocities, as determined by the recoil and by the pendulum, $= 2109 - 1763 = 346$ feet in a ſecond, is extremely near 335 feet in a ſecond, the diminution of the velocity by the reſiſtance as here determined.

If the diminution of the velocities of the bullets in the two ſubſequent experiments be computed in like manner, it will turn out in the 86th experiment $= 65$ feet in a ſecond, and in the 87th experiment $= 33$ feet in a ſecond; and making theſe corrections, the compariſon of the two methods of aſcertaining the velocities will ſtand thus:

	85th exp.	86th exp.	87th exp.
Velocities ſhewn by the pendulum,	1763	1317	1136
Add the diminution of the velocity by the reſiſtance of the air,	335	65	33
	2098	1382	1169
Velocity by the recoil, -	2109	1430	1288
The difference =	+11	+48	+119

So that it appears, notwithſtanding theſe corrections, that the velocities in the 86th and 87th experiments, and particularly in the laſt, as they were determined by the pendulum, are ſtill

ſtill conſiderably deficient. But the manifeſt irregularity of the velocities in thoſe inſtances affords abundant reaſon to conclude, that it muſt have ariſen from ſome extraordinary accidental cauſe, and therefore, that little dependance is to be put upon the reſult of thoſe experiments. I cannot take upon me to determine poſitively what the cauſe was which produced this irregularity; but I ſtrongly ſuſpect, that it aroſe from the breaking of the bullets in the barrel by the force of the explo-ſion: for theſe bullets, as has already been mentioned, were formed of lead, incloſing leſſer bullets of plaſter of Paris; and I well remember to have obſerved at the time ſeveral ſmall fragments of the plaſter which had fallen down by the ſide of the pendulum. I confeſs, I did not then pay much attention to this circumſtance, as I naturally concluded, that it aroſe from the breaking of the bullet in penetrating the target of the pendulum, and that the ſmall pieces of plaſter I ſaw upon the ground had fallen out of the hole by which the bullet entered. But if the bullets were not abſolutely broken in pieces in firing, yet, if they were conſiderably bruiſed, and the plaſter or a part of it were ſeparated from the lead, ſuch a change in their form might produce a great increaſe of the reſiſtance, and even their initial velocities might be affected by it, for their form being changed from that of a globe to ſome other figure, they might not fit the bore, and a part of the force of the charge might be loſt by the windage.

That this actually happened in the 87th experiment ſeems very probable, as the velocity with which the bullet was pro-jected, as it was determined by the recoil, is conſiderably leſs in proportion in that experiment than in either of thoſe that precede it in that ſet, or in thoſe which follow it, as will ap-

pear upon inſpecting the curvature of the line *m*, *n*, fig. 16. But I forbear to inſiſt further upon this matter.

As I have made an allowance for the reſiſtance of the air in theſe experiments, it may be expected that I ſhould do it in all other caſes; but, I think, it will appear upon enquiry, that the diminution of the velocities of the bullets on that account was in general ſo inconſiderable that it might ſafely be neglected: thus, for inſtance, in the experiments with an ounce of powder, when the velocity of the bullet was more than 1750 feet in a ſecond, the diminution turns out no more than about 25 or 30 feet in a ſecond, though we ſuppoſe the full reſiſtance to have begun ſo near as two feet from the mouth of the piece; and in all caſes where the velocities were leſs, the effect of the reſiſtance was leſs in a much greater proportion: and even in this inſtance there is reaſon to think, that the diminution of the velocity as we have determined it is too great; for the flame of gun-powder expands with ſuch an amazing rapidity, that it is ſcarcely to be ſuppoſed but that it follows the bullet, and continues to act upon it more than two feet, or even four feet, from the gun, and when the velocity of the bullet is leſs, its action upon it muſt be ſenſible at a ſtill greater diſtance.

With 218 grains of powder the recoil appears to have been very uniform; but if the velocities of the bullets are determined from the recoil in the 40th and ſeven following experiments, when this charge was made uſe of, and from the recoil without a bullet in the 72d and 73d experiments, the velocities will turn out conſiderably too ſmall, as we ſhall ſee by making the computation.

The

	Vent at 0.		Vent at 1,3.
The recoil in the	40th exp. was 18,	and in the 43d exp. it was	18,3
	41ſt - 17,71	- 44th -	18,35
	42d - 17,91	- 45th -	18,35
	47th - 8,1	- 46th -	18,35
	4)71,72		4)73,35
Means =	17,93	and	18,34

And in the 72d and 73d experiments the recoil, with the ſame charge without a bullet, was 8,72, and 8,47 = 8,595 at a medium, the velocities therefore turn out,

	Vent at 0.	Vent at 1,3.
By the recoil	1105 -	1153
inſtead of	1225 and	1276 as they were ſhewn by the pend.

The difference 120 and 123 feet in a ſecond amounts to near one twelfth part of the whole velocity.

This difference is undoubtedly owing to the recoil without a bullet being taken too great; for it is not only greater than it ought to be, in order that the velocities of the bullets may come out right; but it is conſiderably greater in proportion than the recoil with any other charge.

Thus, with 145 grains of powder the recoil was 4,4
with 165 grains - - it was 5,55
290 grains - - - 10,66
330 grains - - - 12,7
and with 437½ grains - - - it was 17,9

And if the recoil with 218 grains is determined from theſe numbers by interpolation, it comes out 7,5; and with that

value for C the velocities of the bullets in the before mentioned experiments appear to be,

	Vent at 0.		Vent at 1,3.
	1243	and	1283 by the recoil
which is extremely near	1225	and	1276 the velocities

shewn by the pendulum.

It is to be remembered, that the 72d and 73d experiments, from which we before determined the recoil with the given charge of powder without a bullet, were not made upon the same day with the experiments before mentioned; and it is well known, that the force of powder is different upon different days. And it is worthy of remark, that in those two experiments the strength of government powder appeared to be considerably the greatest. I mention these circumstances to shew the probability there is, that the recoil in those experiments, from some unknown cause, was greater than it ought to have been, or rather than it would have been, had the experiments been made at the same time when the experiments with the bullets were made; or at any other time under the same circumstances.

As this method of determining the velocities of the bullets did not occur to me till after I had finished the course of my experiments, and had taken down my apparatus, I have not had an opportunity of ascertaining the recoil, with and without a bullet, with that degree of precision that I could wish. If I had thought of it sooner, or if I had recollected that passage in Mr. ROBINS's new Principles of Gunnery, where he says, "The part of the recoil, arising from the expansion of "the powder alone, is found to be no greater when it impels "a leaden bullet before it, than when the same quantity is fired "without any wad to confine it:" I say, if that passage had occurred

occurred to me before it had been too late, I certainly ſhould have taken ſome pains to have aſcertained the fact; but as it is, I think, enough has been done to ſhew, that there is the greateſt probability that the velocities of bullets may in all caſes be determined by the recoil with great accuracy; and I hope ſoon to have it in my power to put the matter out of all doubt, and to verify this new method by a courſe of concluſive experiments which I am preparing for that purpoſe.

In the mean time I would juſt obſerve, that if this method ſhould be found to anſwer, when applied to muſket bullets, it cannot fail to anſwer equally well when it is applied to cannon balls and bomb ſhells of the largeſt dimenſions; and it is apprehended, that it will be much preferable to any method hitherto made public; not only as it may be applied indifferently to all kinds of military projectiles, and that with very little trouble or expence in making the experiment; but alſo becauſe by it the velocities with which bullets are *actually projected* are determined; whereas by the pendulum their velocities can only be aſcertained at ſome diſtance from the gun, and after they have loſt a part of their initial velocities by the reſiſtance of the air through which they are obliged to paſs to arrive at the pendulum.

At the trifling expence of ten or fifteen pounds an apparatus might be conſtructed that would anſwer for making the experiments with all the different kinds of ordnance in the Britiſh ſervice. The advantages that might be derived from ſuch a ſet of experiments are too obvious to require being mentioned.

Of

Of a very accurate method of proving gun-powder.

All the *eprouvettes*, or powder-triers, in common uſe are defective in many reſpects. Neither the abſolute force of gunpowder can be determined by means of them, nor the comparative force of different kinds of it, but under circumſtances very different from thoſe in which the powder is made uſe of in ſervice.

As the force of powder ariſes from the action of an elaſtic fluid that is generated from it in its inflammation, the quicker the charge takes fire, the more of this fluid will be generated in any given ſhort ſpace of time, and the greater of courſe will be its effect upon the bullet. But in the common method of proving gun-powder, the weight by which the powder is confined is ſo great in proportion to the quantity of the charge, that there is time quite ſufficient for the charge to be all inflamed, even when the powder is of the ſloweſt compoſition, before the body to be put in motion can be ſenſibly removed from its place. The experiment, therefore, may ſhew which of two kinds of powder is the ſtrongeſt, when equal quantities of both are confined in equal ſpaces, and completely inflamed; but the degree of the inflammability, which is a property eſſential to the goodneſs of the powder, cannot by theſe means be aſcertained.

Hence it appears, how powder may anſwer to the proof, ſuch as is commonly required, and may nevertheleſs turn out very indifferent when it comes to be uſed in ſervice. And this, I believe, frequently happens; at leaſt I know complaints from officers of the badneſs of our powder are very common; and I would ſuppoſe that no powder is ever received by the

Board of Ordnance but ſuch as has gone through the eſtabliſhed examination, and has anſwered to the uſual teſt of its being of the ſtandard degree of ſtrength.

But though the common powder-triers may ſhew powder to be better than it really is, they never can make it appear to be worſe than it is; it will therefore always be the intereſt of thoſe who manufacture that commodity to adhere to the old method of proving it. But the purchaſer will find his account in having it examined in a method by which its goodneſs may be aſcertained with greater preciſion.

The method I would recommend is as follows. A quantity of powder being provided, which, from any previous examination or trial, is known to be of a proper degree of ſtrength to ſerve as a ſtandard for the proof of other powder, a given charge of it is to be fired, with a fit bullet, in a barrel ſuſpended by two pendulous rods, according to the method before deſcribed, and the recoil is to be carefully meaſured upon the ribbon. And this experiment being repeated three or four times; or oftener if there is any difference in the recoil, the mean and the extremes of the chords may be marked upon the ribbon by black lines drawn acroſs it, and the word *proof* may be written upon the middle line; or if the recoil is uniform (which it will be to a ſufficient degree of accuracy, if care is taken to make the experiments under the ſame circumſtances) then the *proof mark* is to be made in that part of the ribbon to which it was conſtantly drawn out by the recoil in the different trials.

The recoil, with a known charge of ſtandard powder, being thus aſcertained and marked upon the ribbon, let an equal quantity of any other powder (that is to be proved) be fired in the ſame barrel, with a bullet of the ſame weight, and every other circumſtance alike, and if the ribbon is drawn out as far

far or farther than the proof mark, the powder is as good or better than the ſtandard; but if it falls ſhort of that diſtance, it is worſe than the ſtandard, and is to be rejected.

For the greater the velocity is with which the bullet is impelled, the greater will be the recoil; and when the recoil is the ſame, the velocities of the bullets are equal, and the powder is of the ſame degree of ſtrength, if the quantity of the charge is the ſame. And if care is taken in proportioning the charge to the weight of the bullet, to come as near as poſſible to the medium proportion that obtains in practice, the determination of the goodneſs of gun-powder from the reſult of this experiment cannot fail to hold good in actual ſervice.

Fig. 14. repreſents the propoſed apparatus, drawn to a ſcale of one foot to the inch. *a*, *b*, is the barrel ſuſpended by the pendulous rods *c*, *d*; and *r* is the ribbon for meaſuring the recoil.

The length of the bore is 30 inches, and its diameter is one inch, conſequently it is juſt 30 calibres in length, and will carry a leaden bullet of about 3 ounces.

The barrel may be made of gun metal, or of caſt iron as that is a cheaper commodity; but great care muſt be taken in boring it, to make the cylinder perfectly ſtrait and ſmooth, as well as to preſerve the proper dimenſions. Of whatever metal the barrel is made, it ought to weigh at leaſt 50 lbs. in order that the velocity of the recoil may not be too great; and the rods by which it is ſuſpended ſhould be five feet in length. The vent may be about one twentieth of an inch in diameter; and it ſhould be *bouched* or lined with gold, in the ſame manner as the touch-hole is made in the better kind of fowling pieces, in order that its dimenſions may not be increaſed by repeated firing.

The bullets ſhould be made to fit the bore with very little windage; and it would be better if they were all caſt in the ſame mould, and of the ſame parcel of lead, as in that caſe their weights and dimenſions would be more accurately the ſame, and the experiments would of courſe be more concluſive.

The ſtated charge of powder may be half an ounce, and it ſhould always be put up in a cartridge of very fine paper; and after the piece is loaded it ſhould be primed with other powder, firſt taking care to prick the cartridge by thruſting a priming wire down the vent.

As it appears, from ſeveral experiments made on purpoſe to aſcertain the fact, that ramming the powder more or leſs has a very ſenſible effect to increaſe or diminiſh the force of the charge; to prevent any inaccuracies that might ariſe from that cauſe, a ram-rod, ſuch as is repreſented fig. 15. may be made uſe of. It is to be made of a cylindric piece of wood in the ſame manner as ram-rods in general are made, but with the addition of a ring C, about one inch and a half, or two inches in diameter, which, being placed at a proper diſtance from the end (*a*) of the ram-rod that goes up into the bore, will prevent its being thruſt up too far. This ring may be made of wood, or of any kind of metal as ſhall be found moſt convenient. The other end of the ram-rod (*b*) may be 31 or 32 inches in length from the ring, and the extremity of it being covered with a proper ſubſtance, it may be made uſe of for wiping out the barrel after each experiment.

The machine (*f*) for the tape to ſlide through may be the ſame as is deſcribed by Dr. HUTTON in his account of his experiments on the initial velocities of cannon balls; as his method is much better calculated to anſwer the purpoſe than that propoſed and made uſe of by Mr. ROBINS. It will alſo be

better for the axis of the pendulous rods to reſt upon level pieces of wood or iron, than for them to move in circular grooves: only care muſt be taken to confine them by ſtaples or ſome other contrivance, to prevent their ſlipping out of their places.

The trunnions, by means of which the barrel is connected with the pendulous rods, and upon which it is ſupported, ſhould be as ſmall as poſſible, in order to leſſen the friction; and for the ſame reaſon they ſhould be well poliſhed, as well as the grooves that receive them. They need not be caſt upon the barrel, but may be ſcrewed into it after it is finiſhed.

In making the experiments, regard muſt be had to the heat of the barrel, as well as to the temperature of the atmoſphere; for heat and cold, dryneſs and moiſture, have a very ſenſible effect upon gun-powder to increaſe or diminiſh its force. If therefore a very great degree of accuracy is at any time required, it will be beſt to begin by firing the piece two or three times merely to warm it; after which three or four experiments may be made with ſtandard powder, to determine anew the proof mark (for the ſtrength of the ſame powder is different upon different days); and when this is done, the experiments with the powder that is to be proved are to be made, taking care to preſerve the ſame interval of time between the firings, that the heat of the piece may be the ſame in each trial.

If all theſe precautions are taken, and if the bullets are of the ſame weight and dimenſions, powder may be proved by this method with much greater accuracy than has hitherto been done by any of the common methods made uſe of for that purpoſe.

Of the comparative goodneſs, or value, of powder of different degrees of ſtrength.

Let V denote the velocity of the bullet with the ſtronger powder, and put *v* equal to the velocity with the weaker, when the charges are equal, and the weight and dimenſions of the bullets are the ſame, and when they are diſcharged from the ſame piece. If the charge is augmented when the weaker powder is made uſe of, till the velocity of the bullet is increaſed from *v* to V, or becomes equal to the velocity with the given charge of the ſtronger powder, the *value* of the charges may then be ſaid to be equal; and conſequently the weaker powder is as much worſe than the ſtronger, or is of leſs value in proportion as the quantity of it required by the pound, to produce the given effect is greater.

But it is well known, that the velocities, with different quantities of the ſame kind of powder, are in the ſub-duplicate ratio of the weights of the charges. The charges, therefore, muſt be as the ſquares of the velocities, and conſequently the charge of the weaker powder muſt be to that of the ſtronger, when the velocities are equal, as VV is to *vv*. The weaker powder is therefore as much worſe than the ſtronger as VV is greater than *vv*; or the comparative goodneſs of powder, of different degrees of ſtrength, is as the ſquares of the velocities of the bullets when the charges are equal.

The mean velocity of the bullets, as ſhewn by the pendulum in the 104th and 105th experiments, when the piece was fired with 145 grains of government powder, was 894 feet

in a ſecond; and with the ſame quantity of double proof * battle powder (experiment N° 106) the velocity was 990 feet in a ſecond. Now the ſquares of theſe velocities, which, as we juſt obſerved, meaſure the goodneſs of the powder, are to each other as 1 is to 1,2263, or nearly as 5 is to 6.

With 218 grains of government powder, the mean velocity in four experiments (*viz.* the 40th, 41ſt, 42d, and 43d) was 1225 feet in a ſecond; and in the experiment N° 107. when the ſame quantity of double proof battle powder was made uſe of, the velocity was 1380 in a ſecond; and $\overline{1225}$ is to $\overline{1380}$ as 1 is to 1,2691.

With 290 grains, or half the weight of the bullet in government powder in the 109th, 110th, 111th, and 112th experiments, the mean velocity of the bullet was 1444 feet in a ſecond; but with the ſame quantity of the battle powder (experiment N° 116.) the velocity was 1525 feet in a ſecond; $\overline{1444}$ is to $\overline{1525}$, as 1 is to 1,1153.

By taking a medium of theſe trials it appears, that double proof battle powder is better than government powder in proportion as 1,2036 is to 1, or nearly as 6 is to 5.

But if, inſtead of weighing the powder, we eſtimate the quantity of the charge by meaſurement, or the ſpace it occupies in the bore of the piece, the comparative ſtrength of battle powder will appear to be conſiderably greater, or its ſtrength will be to that of government powder nearly as 4 is to 3; for the grains of this better kind of powder being more compact and nearly of a ſpherical form, a greater weight of it will lie in any

* This is called *battle* powder, not becauſe it is uſed in battle or in war; but from *Battle*, the name of a village in Kent, where that kind of powder is made.

given ſpace than of government powder, which is formed more looſely, and of various and of very irregular figures.

Now the common price of double proof battle powder, as it is ſold by the wholeſale dealers in that commodity, is at the rate of £. 10 *per* cwt. net, which is juſt two ſhillings by the pound; while government powder is ſold at £. 5 5*s*. *per* hundred, or one ſhilling and $\frac{5}{10}$th of a penny *per* pound; but battle powder is better than government powder only in the proportion of 1,2036 to 1, or of one ſhilling and two pence to one ſhilling and $\frac{5}{10}$th of a penny; battle powder is therefore ſold at the rate of ten pence by the pound, or 41$\frac{2}{3}$ *per cent.* dearer than it ought to be; or thoſe, who make uſe of it in preference to government powder, do it at a certain loſs of 41$\frac{2}{3}$ *per cent.* of the money that the powder coſts them.

Of the relation of the velocities of bullets to their weights.

According to Mr. ROBINS's theory, when bullets of the ſame diameter, but different weights, are diſcharged from the ſame piece by the ſame quantity of powder, their velocities ſhould be in the reciprocal ſub-duplicate ratio of their weights; but as this theory is founded upon a ſuppoſition that the action of the elaſtic fluid, generated from the powder, is always the ſame in any and every given part of the bore when the charge is the ſame, whatever may be the weight of the bullet; and as no allowance is made for the expenditure of force required to put the fluid itſelf in motion, or for the loſs of it by the vent; it is plain that the theory is defective. It is true, Dr. HUTTON in his experiments found this law to obtain without any great error, and poſſibly it may hold good with ſufficient accuracy in many caſes; for it ſometimes happens that a number of errors or

actions,

actions, whoſe operations have a contrary tendency, ſo compenſate each other, that their effects when united are not ſenſible. But when this is the caſe, if any one of the cauſes of error is removed, thoſe which remain will be detected.

When any given charge is loaded with a heavy bullet, more of the powder is inflamed in any very ſhort ſpace of time than when the bullet is lighter, and the action of the powder ought of courſe to be greater on that account; but then a heavy bullet takes up more time in paſſing through the bore than a light one, and conſequently more of the elaſtic fluid, generated from the powder, eſcapes by the vent and by windage. It may happen, that the augmentation of the force, on account of one of theſe circumſtances, may exactly counterbalance the diminution of it ariſing from the other; and if it ſhould be found upon trial that this is the caſe in general, in pieces as they are now conſtructed, and with all the variety of ſhot that are made uſe of in practice, it would be of great uſe to know the fact: and poſſibly it might anſwer as well, as far as it relates to the art of gunnery, as if we were perfectly acquainted with, and were able to appreciate, the effect of each varying circumſtance under which an experiment can be made. But when, concluding too haſtily from the reſult of a partial experiment, we ſuppoſe, with Mr. ROBINS, that becauſe the ſum total of the action or preſſure of the elaſtic fluid upon the bullet, during the time of its paſſage through the bore, happens to be the ſame when bullets of different weights are made uſe of (which collective preſſure is in all caſes proportional to, and is accurately meaſured by, the velocity, or rather motion, communicated to the bullet) that therefore the preſſure in any given part is always exactly the ſame, when the quantity of powder is the ſame with which the piece is fired; and from thence endeavour to

prove,

prove, that the inflammation of gun-powder is inſtantaneous, or that the whole charge is in all caſes inflamed, and "converted into an elaſtic fluid before the bullet is ſenſibly moved "from its place;" ſuch reaſonings and concluſions may lead to very dangerous errors.

It is undoubtedly true, that if the principles aſſumed by Mr. ROBINS with reſpect to the manner in which gun-powder takes fire, and the relation of the elaſticity of the generated fluid to its denſity, or the intenſity of its preſſure upon the bullet as it expands in the barrel, were juſt, and if the loſs of force by the vent and windage was in all caſes inconſiderable, or if it was prevented, the deductions from the theory reſpecting the velocities of bullets of different weights would always hold good. But if, on the contrary, it ſhould be found upon making the experiments carefully, and in ſuch a manner as entirely to prevent inaccuracies ariſing from adventitious circumſtances, that the velocities obſerve a law different from that which the theory ſuppoſes, we may fairly conclude, that the principles upon which the theory is founded are erroneous.

Let us now ſee how far theſe experiments differ from the theory. Thoſe numbered from 84 to 92 inclusive were made in ſuch a manner that no part of the force of the powder was loſt by the vent, or by windage, as has already been mentioned, and all poſſible attention was paid to every circumſtance that could contribute to render them perfect and concluſive.

A particular account of them with the means uſed for forming the bullets, and making them fit for the bore, and the contrivance for preventing the eſcape of the elaſtic fluid by the vent, &c. may be ſeen in the general table, p. 245. The following table ſhews the reſult of them.

N. B. The charge of powder was the ſame in each experiment, and conſiſted of 145 grains in weight.

	Weight of the bullet.	Velocity of the bullet. Actual.	Computed.	Difference.
85th exp.	90 grs.	2109	2109	——
86th	251	1430	1262	+168
87th	354	1288	1063	+225
88th	600	1240	817	+423
89th	603	1224	815	+409
90th	1184	1017	581	+436
91ſt	1754	893	478	+415
92d	2352	812	413	+399

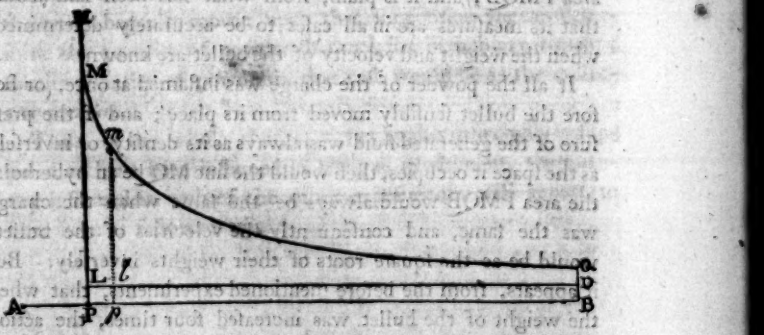
The computed velocities, as they are ſet down in this table, were determined from the actual velocity of the bullet, as determined by the recoil in the 85th experiment; and the reciprocal ſub-duplicate ratio of its weight to the weight of the bullet in each ſubſequent experiment; and in the laſt column is marked the difference between the experiment and the theory, or the number of feet in a ſecond, by which the actual velocity exceeds the computed.

But in order that we may ſee this matter in different points of view, let the order of the experiments be now inverted, and let the computed velocities be determined from the actual velocity in the 92d experiment; and aſſuming the total or collective preſſure exerted by the powder upon the bullet in that experiment equal to unity, let the collective preſſure in the other experiments be computed from the ratio of the actual to the computed velocities, and the table will ſtand thus:

92d

	Weight of the bullet.	Velocity of the bullet			Collective pressure.
		Actual.	Computed.	Difference.	
92d exp.	2352 grs.	812	812	——	1,0000
91ſt	1754	893	940	– 47	0,9020
90th	1184	1017	1145	– 128	0,7897
89th	603	1224	1604	– 380	0,5825
88th	600	1240	1608	– 368	0,5949
87th	354	1288	2093	– 805	0,3778
86th	251	1430	2486	– 1056	0,3310
85th	90	2109	4151	– 2042	0,2581

In the following figure let AB repreſent the axis of the piece, and AP the length of the ſpace filled with powder; and at the



point P let the perpendicular PH be erected, upon which let PL and PM be taken from P towards H of ſuch magnitudes that while PL expounds the uniform force of gravity, or the weight of the bullet, PM ſhall be as the force exerted by the powder

upon the bullet at the moment of the explosion. If now we suppose, that while the bullet moves on from P towards B, the line PM or *pm*, goes along with it, and that the point *m* is always taken in such a manner that the line *pm*, shall be to *pl*, or PL, as the force acting upon the bullet in the point *p*, is to its weight, till *pm*, coincides with QB, then will the area PMQB be to the area PLDB in the duplicate proportion of the velocities which the bullet would acquire when acted on by its own gravity through the space PB, and when impelled through the same space by the force of the powder, as may be seen demonstrated by Sir ISAAC NEWTON, in his Mathematical Principles of Natural Philosophy, book I. prop. 39.

Now what I call the collective pressure, or sum total of the action of the powder upon the bullet, is the measure of the area PMQB; and it is plain, from what has been said above, that its measures are in all cases to be accurately determined, when the weight and velocity of the bullet are known.

If all the powder of the charge was inflamed at once, or before the bullet sensibly moved from its place; and if the pressure of the generated fluid was always as its density, or inversely as the space it occupies, then would the line MQ be an hyperbola, the area PMQB would always be the same when the charge was the same, and consequently the velocities of the bullets would be as the square roots of their weights inversely. But it appears, from the before mentioned experiments, that when the weight of the bullet was increased four times, the action of the powder, or area PMQB, was nearly doubled; for in the 92d experiment, when four bullets were discharged at once, the collective pressure was as 1; but in the 89th experiment, when a single bullet was made use of, the collective

pressure

preſſure was only as 0,5825; and, in the 85th, 86th, and 87th experiments, when the bullets were much lighter, the action of the charge was ſtill leſs.

But though we can determine with great certainty, from theſe experiments, the ratio in which the action of the powder upon the bullet was increaſed or diminiſhed, by making uſe of bullets of greater or leſs weight; yet we cannot from thence aſcertain the relation of the elaſticity of the generated fluid to its denſity, nor the quantity of powder that is inflamed at different periods before and after the bullet begins to move in the bore.

But aſſuming Mr. ROBINS's principles as far as relates to the elaſticity of the fluid, and ſuppoſing that in all the experiments, except the 92d, a part only of the charge took fire, and that that part was inflamed and converted into an elaſtic fluid before the bullet began to move; upon that ſuppoſition we can determine the quantity of powder that took fire in each experiment; for the quantity of powder in that caſe would be as the collective preſſure.

Thus, if the whole charge, = 145 grains in weight, is ſuppoſed to have been inflamed in the 92d experiment, the quantity inflamed in each of the other experiments will appear to have been as follows; *viz.*

	Weight of the bullets.	Velocity of the bullet.	Collective preſſure.	Powder inflamed.
85th exp.	90 grs.	2109	0,2581	37 grs.
86th	251	1430	0,3310	48
87th	354	1288	0,3778	55
88th	600	1240	0,5949	86
89th	603	1224	0,5825	84
90th	1184	1017	0,7897	114
91ſt	1754	893	0,9020	131

But there are many reaſons to ſuppoſe, that the diminution of the action of the powder upon the bullet, when it is lighter, is not ſo much owing to the ſmalleſs of the quantity of powder that takes fire in that caſe as to the *vis inertiæ* of the generated fluid. It is true, that a greater portion of the charge takes fire when the bullet is heavy than when it is light, as I found in the very experiments of which I am now ſpeaking; but then the quantity of unfired powder in any caſe was much too ſmall to account for the apparent diminution of the force when light bullets were made uſe of.

If the elaſtic fluid, in the action of which the force of powder conſiſts, were infinitely fine, or if its weight bore no proportion to that of the powder that generated it; and if the groſs matter, or *caput mortuum*, of the powder remained in the bottom of the bore after the explosion, then, and upon no other ſuppoſition, would the preſſure upon the bullet be inverſely as the ſpace occupied by the fluid: but it is evident that this can never be the caſe.

A curious ſubject for ſpeculation here occurs: how far would it be advantageous, were it poſſible, to diminiſh the ſpecific gravity of gun-powder, and the fluid generated from it, with-

out leſſening its elaſtic force? It would certainly act upon very light bullets with greater force; but when heavy ones came to be made uſe of, there is reaſon to think, that, except extraordinary precaution was taken to prevent it, the greateſt part of the force would be loſt by the vent and by windage.

The velocity with which elaſtic fluids ruſh into a void ſpace is as the elaſticity of the fluid directly, and inverſely as its denſity; if, therefore, the denſity of the fluid generated from powder was four times leſs than it is, its elaſticity remaining the ſame, it would iſſue out at the vent, and eſcape by the ſide of the bullet in the bore, with nearly four times as great a velocity as it does at preſent; but we know from experiment that the loſs of force on thoſe accounts is now very conſiderable.

In the experiments N° 76. and 77. when the piece was fired with 145 grains of powder, the velocity of the bullets at a medium was 1040 feet in a ſecond; but in the 88th and 89th experiments, when the bullets were even heavier, and the piece was fired with the ſame quantity of powder, the mean velocity was 1232 feet in a ſecond. The difference = 192 feet in a ſecond, anſwers to a difference of force greater in the laſt experiments than in the firſt in the proportion of 14 to 10.

I know of no way to account for this difference, but by ſuppoſing that it was owing intirely to the eſcape of the elaſtic fluid by the vent, and by windage, in thoſe experiments where the vent was open, and the bullets were put naked into the piece.

An elaſtic bow, made of very light wood, will throw an arrow, and eſpecially a light one, with greater velocity than a bow of ſteel of the ſame degree of ſtiffneſs: but, for practice, I think it is plain, that gun-powder may *be ſuppoſed* to be ſo

light

light as to be rendered intirely uſeleſs: and for ſome purpoſes it ſeems probable, that it would not be the worſe for being even heavier than it is now made. Vents are abſolutely neceſſary in fire-arms, and in large pieces the windage muſt be conſiderable, in order that the bullets, which are not always ſo round as they ſhould be, may not ſtick in the bore; and thoſe who have been preſent at the firing of heavy artillery and large mortars with ſhot and ſhells, muſt have obſerved, that there is a ſenſible ſpace of time elapſes between the lighting of the prime and the exploſion; and that, during that interval, the flame is continually iſſuing out at the vent with a hiſſing noiſe, and with a prodigious velocity, as appears by the height to which the ſtream of fire mounts up in the air. It is plain, that this loſs muſt be greater in proportion as the ſhot that is diſcharged is heavier; and I have often fancied, that I perceived a ſenſible difference in the time that elapſed between the firing of the prime and the exploſion, when bullets were diſcharged, and when the piece has been fired with powder only; the time being apparently longer in the former caſe than the latter.

Almoſt all the writers upon gun-powder, and particularly thoſe of the laſt century, gave different recipes for powder that is deſigned for different uſes. Thus the French authors mention *poudre à mouſquet*, *poudre ordinaire de guerre*, *poudre de chaſſe*, and *poudre d'artifice*; all of which are compoſed of ſalt petre, ſulphur, and charcoal, taken in different proportions. Is it not probable, that this variety in the compoſition of powder was originally introduced in conſequence of obſervation that one kind of powder was better adapted for particular purpoſes than another, or from experiments made on purpoſe to aſcertain the fact? There is one circumſtance that would lead us to ſuppoſe that that was the caſe.—That kind of pow-

der

der which was designed for great guns and mortars was weaker than those which were intended to be used in smaller pieces: for if there is any foundation for these conjectures, it is certain, that the weakest powder, or the heaviest in proportion to its elastic force, ought to be used to impel the heaviest bullets, and particularly in guns that are imperfectly formed, where the vent is large, and the windage very great.

I am perfectly aware, that an objection may here be made, viz. that the elastic fluid, which is generated from gun-powder, must be supposed to have the same properties very nearly, whatever may be the proportion of the several ingredients, and that therefore the only difference there can be in powder is, that one kind may generate more of this fluid, and another less; and that when it is generated, it acts in the same manner, and will alike escape, and with the same velocity, by any passage it can find. But to this I answer, though the fluid may be the same, as undoubtedly it is, and though its density and elasticity may be the same in all cases at the instant of its generation, yet in the explosion, the elastic and un-elastic parts are so mixed and blended, that I imagine the fluid cannot expand without taking the gross matter along with it, and the velocity with which the flame issues out at the vent is to be computed from the elasticity of the fluid, and the density or weight of the fluid and the gross matter taken together, and not simply from the elasticity and density of the fluid. If antimony in an impalpable powder, or any other heavy body, was intimately mixed with water in a vessel of any kind, and kept in suspension by shaking or stirring them about; and if a hole was opened in the side or bottom of the vessel, the water would not run out without taking the particles of the solid body along with it. And in the same manner I conceive the solid particles that

remain

remain after the explosion of gun-powder are carried forward with the generated elastic fluid, and being carried forward retard its motion.—But to return from this digression.

As it appears from these experiments, that the relation of the velocities of bullets to their weights is different from that which Mr. ROBINS's theory supposes, it remains to inquire what the law is which actually obtains. And, first, as the velocities bear a greater proportion to each other than the reciprocal sub-duplicate ratio of the weights of the bullets, let us see how near they come to the reciprocal sub-triplicate ratio of their weights.

Weight of the bullet.	Velocity of the bullet					
	Computed.		Actual.		Computed.	
	Recip. sub-dup. ratio.	Error of the theory.		Error of the theory.	Recip. sub-trip. ratio.	
92d	2352	812		812		812
91st	1754	940	+ 47	893	+ 2	895
90th	1184	1145	+ 128	1017	+ 4	1021
89th	603	1604	+ 380	1224	+ 54	1278
88th	600	1608	+ 368	1240	+ 40	1280
87th	354	2093	+ 805	1288	+ 239	1527
86th	251	2486	+ 1056	1430	+ 282	1712
85th	90	4151	+ 2042	2109	+ 301	2410

Here the velocities computed upon the last supposition appear to agree much better with the experiments than those computed upon Mr. ROBINS's principles; but still there is a considerable difference between the actual and the computed velocities in the three last experiments in the table.

As the powder itself is heavy, it may be considered as a weight that is put in motion along with the bullet; and if we

suppose

suppose the density of the generated fluid is always uniform from the bullet to the breech, the velocity of the center of gravity of the powder or (which amounts to the same thing) of the elastic fluid, and the gross matter generated from it will be just half as great as the velocity of the bullet. If therefore we put P to denote the weight of the powder, B the weight of the bullet, and v its initial velocity; then $Bv + \frac{1}{2}Pv = \overline{B + \frac{1}{2}P} \times v$ will express the *momentum* of the charge at the instant when the bullet quits the bore.

If now, instead of ascertaining the relation of the velocities to the weights of the bullets, we add half the weight of the powder to the weight of the bullet, and compute the velocities from the reciprocal sub-triplicate ratio of the quantity $\overline{B + \frac{1}{2}P}$ in each experiment, the table will stand thus:

	Weight of the bullet and half the powder. $\overline{B+\frac{1}{2}P}=$	Velocity of the bullet. Actual.	Computed.	Error of the theory.
92d exp.	$\overline{2352+72\frac{1}{2}}$	812	812	——
91st	$\overline{1754+72\frac{1}{2}}$	893	892	− 1
90th	$\overline{1184+72\frac{1}{2}}$	1017	1011	− 6
89th	$\overline{603+72\frac{1}{2}}$	1224	1243	+ 19
88th	$\overline{600+72\frac{1}{2}}$	1240	1245	+ 5
87th	$\overline{354+72\frac{1}{2}}$	1288	1449	+161
86th	$\overline{251+72\frac{1}{2}}$	1430	1589	+159
85th	$\overline{90+72\frac{1}{2}}$	2109	1999	−110

The agreement between the actual and computed velocities is here very remarkable, and particularly in the five first experiments, which are certainly those upon which the greatest dependence may be placed.

And hence we are enabled to determine the natures of the mn, and gf (fig. 16.); for since B (which expresses the weight of the bullet) is as the length taken from A towards B in the figure in the several experiments; and as the velocities are as the lines drawn perpendicular to the line AB from the places where those lengths terminate, as w, n, &c. ending at the curve m, n; if we put $b = \frac{1}{3}$P, $x =$ B, and $y = wn$; then will the relation of x and y be defined by this equation $\frac{1}{\sqrt{a+x^2}} = y$. And if z be put to denote the line wr, and b, the recoil when the given charge is fired without any bullet, it will be $\frac{x}{\sqrt{a+x^2}} + b = z$ in the curve gf, x being the abscissa, and z the corresponding ordinate to the curve.

In the 9zd experiment half the weight of the powder ($= a$) was 72½ grains; the weight of the bullet was 2352 grains ($= x$); the recoil ($= z$) was 32,25 inches, and with the given charge without any bullet the recoil ($= b$) was 4,4 inches; if now from these data, and the known weight of the bullet in each of the other experiments in this set, the recoil be computed by means of the theorem $\frac{x}{\sqrt{a+x^2}} + b = z$, we shall see how the result of those experiments agrees with this theory, thus,

9zd

	Weight of the bullet.	Recoil: Actual.	Recoil: Computed.	Difference.
92d exp.	2352	32,25	32,25	——
91ſt	1754	27,18	27,22	+0,04
90th	1184	21,92	21,85	−0,07
89th	603	15,13	15,33	+0,2
88th	600	15,22	15,29	+0,07
87th	354	11,03	11,87	+0,84
86th	251	9,62	10,21	+0,59
85th	90	7,16	7,02	−0,14
84th and 93d	0	4,4	4,4	——

Here the agreement of the actual and computed recoils is as remarkable as that of the actual and computed velocities in the foregoing table.

By the figure 17. may be ſeen at one view the reſult of all theſe experiments and computations. The numbers upon the line AB (as in the fig. 16.) repreſent the weights of the bullets, while the lines drawn from thoſe numbers perpendicular to AB on each ſide, and ending at the curves *m*, *n*, are as the velocities of the bullets in the ſeveral experiments; the line AB being the axis of the curves, the lengths taken from A to the different numbers towards B ($=x$) the abſciſſa, and the perpendiculars ($=y$) the correſponding ordinates. The ordinates to the the curve *hm*, are as the velocities computed from the theorem $\frac{1}{\sqrt{a+x^3}}=y$, and the ordinates to the curve *p*, *n* (which is the logarithmic curve, as it is $\frac{1}{\sqrt{x}}=y$) ſhew the velocities computed upon Mr. ROBINS's principles. The curve *gf* is drawn from

the theorem $\frac{x}{\sqrt{a+x^2}}+b=z$; and the actual recoil is marked upon the ordinates to this curve by large round dots, which in all the experiments, except the 86th and 87th, very nearly coincide with the curve.

In the fig. 18. the numbers upon the line AB, taken from A, denote the different charges of powder uſed in the courſe of the experiments, while the ordinates to the curve *cd*, expreſs the velocities of the bullets, with the vent at o. The lines drawn perpendicular from the line AB to the line *ef*, repreſent the recoil with the ſeveral charges of powder, and a leaden bullet; and the portion of thoſe lines that is comprehended between the line AB and the line *gb*, denotes the recoil when the given charge was fired without any bullet.

Having now ſhewn by experiment the relation of the velocities of bullets to their weights, when care is taken to prevent intirely the loſs of force by the eſcape of the elaſtic fluid through the vent and by the windage, I ſhall leave it to mathematicians to determine from theſe *data* the properties of that fluid.

But, before I take my leave of this ſubject, I would juſt obſerve, that Mr. ROBINS is not only miſtaken in the principle he aſſumes, reſpecting the relation of the elaſticity of the fluid generated from gun-powder to its denſity, or rather the law of its action upon the bullet as it expands in the bore; but his determination of the force of gun-powder is alſo erroneous, even upon his own principles: for he determines its force to be 1000 times greater than the mean preſſure of the atmoſphere; whereas it appears, from the reſult of the 92d experiment, that its force is at leaſt 1308 times greater than the mean

mean preſſure of the atmoſphere, as will be evident to thoſe who will take the trouble to make the computation.

Of an attempt to determine the exploſive force of aurum fulminans, *or a compariſon between its force and that of gun-powder.*

Having provided myſelf with a ſmall quantity of this wonderful powder, upon the goodneſs of which I could depend, I endeavoured to aſcertain its exploſive force by making uſe of it inſtead of gun-powder for diſcharging a bullet, and meaſuring, by means of the pendulum, the velocity which the bullet acquired; and concluding, from the tremendous report with which this ſubſtance explodes, that its elaſtic force was vaſtly greater than that of gun-powder, I took care to have a barrel provided of uncommon ſtrength, on purpoſe for the experiment. Its length in the bore is 13,25 inches, the diameter of the bore is 0,55 of an inch, and its weight 7 lbs. 2 oz. It is of the beſt iron, and was made by WOGDON; and the accuracy with which it is finiſhed does credit to the workman.

This barrel being charged with one ſixteenth of an ounce (=27,34 grains) of *aurum fulminans* and two leaden bullets, which, together with the leather that was put about them to make them fit the bore without windage, weighed 427 grains; it was laid upon a chaffing-diſh of live coals, at the diſtance of about 10 feet from the pendulum, and againſt the center of the target of the pendulum the piece was directed.

Having ſecured the barrel in ſuch a manner that its direction ſhould not alter, I retired to a little diſtance, in order to be out of danger in caſe of an accident, where I waited in anxious expectation the event of the exploſion.

I had

I had remained in this ſituation ſome minutes, and almoſt deſpaired of the experiment's ſucceeding, when the powder exploded, but with a report infinitely leſs than what I expected, the noiſe not greatly exceeding the report of a well-charged wind gun; and it was not till I ſaw the pendulum in motion that I could be perſuaded that the bullets had been diſcharged. I found, however, upon examination, that nothing was left in the barrel, and from the great number of ſmall particles of revived metal that were diſperſed about, I had reaſon to think that all the powder had exploded.

The bullets ſtruck the pendulum nearly in the center of the target, and both of them remained in the wood: and I found, upon making the calculation, that they had impinged againſt it with a velocity of 428 feet in a ſecond.

If we now ſuppoſe that the force of *aurum fulminans* ariſes from the action of an elaſtic fluid that is generated from it in the moment of its exploſion, and that the elaſticity of this fluid, or rather the force it exerts upon the bullet as it goes on to expand, is always as its denſity, or inverſely as the ſpace it occupies; then, from the known dimenſions of the barrel, the length of the ſpace occupied by the charge (which in this experiment was 0,47 of an inch), and the weight and velocity of the bullets, the elaſtic force of this fluid at the inſtant of its generation may be determined: and I find, upon making the calculation upon theſe principles, that its force turns out 307 times greater than the mean elaſtic force of common air.

According to Mr. ROBINS's theory, the elaſtic force of the fluid generated from gun-powder in its inflammation is 1,000 times greater than the mean preſſure of the atmoſphere; the force of *aurum fulminans*, therefore, appears to be to that of gun-powder as 307 is to 1,000, or as 4 is to 13 very nearly.

Of the ſpecific gravity of gun-powder.

To determine the ſpecific gravity of gun-powder I made uſe of the following method. A large glaſs bucket, with a narrow mouth, being ſuſpended to one of the arms of a very nice balance, and exactly counter-poiſed by weights put in the oppoſite ſcale, it was filled firſt with government powder poured in lightly, then with the ſame powder ſhaken well together, afterwards with powder and water together, and laſtly with water alone, and in each caſe the contents of the bucket were very exactly weighed.

The ſpecific gravity of gun-powder, as determined from theſe experiments, is as follows:

Specific gravity of rain water - -	1,000
Government powder, as it lies light in a heap, mixed with air - - - -	0,836
Government powder well ſhaken together -	0,937
The ſolid ſubſtance of the powder - -	1,745

Hence it appears, that a cubic inch of government powder ſhaken well together weighs juſt 243 grains; that a cubic inch of ſolid powder would weigh 442 grains; and, conſequently, that the interſtices between the particles of the powder, as it is grained for uſe, are nearly as great as the ſpaces which theſe particles occupy.

MIS-

MISCELLANEOUS EXPERIMENTS.

Of ſome unſucceſsful attempts to increaſe the force of gun-powder.

It has been ſuppoſed by many, that the force of ſteam is even greater than that of gun-powder; and that if a quantity of water, confined in the chamber of a gun, could at once be rarified into ſteam, it would impel a bullet with prodigious velocity. Several attempts have been made to ſhoot bullets in this manner; but I know of none that have ſucceeded; at leaſt ſo far as to render it probable that water can ever be ſubſtituted in the room of gun-powder for military purpoſes, as ſome have imagined.

The great difficulty that attends making theſe experiments lies in finding out a method by which the water can at once be rarified, and converted into elaſtic ſteam; and it occurred to me, that poſſibly that might be effected by means of gun-powder, by confining a ſmall quantity of water in ſome very thin ſubſtance, and ſurrounding and incloſing it with powder, and afterwards ſetting fire to the charge. The method I took to do this was as follows. Having procured a number of air bladders of very ſmall fiſhes, I put different quantities of water into them from the ſize of a ſmall pea to that of a ſmall piſtol bullet, and tying them up cloſe with ſome very fine thread, I hung up theſe little globules in the open air till they were quite dry on the outſide. I then provided a number of cartridges made of fine paper, and filled them with a known quantity of powder, equal to the cuſtomary charge for a common horſeman's piſtol; and having loaded ſuch a piſtol with one of them

and

and a fit bullet, I laid it down upon the ground, and directing it againſt an oaken plank that was placed about ſix feet from the muzzle, I fired it off by a train, and carefully obſerved the recoil, and alſo the penetration of the bullet. I then took ſeveral of the filled cartridges that remained, and pouring out part of the powder, I put one or more of the little bladders filled with water in the center of the cartridge, and afterwards pouring back the remaining part of the charge, confined the water in the midſt of the powder.

With theſe cartridges and a fit bullet, the piſtol was ſucceſſively loaded, and being placed upon the ground as before, and fired by a train, the recoil, and the penetration of the bullets were obſerved; and I conſtantly found, that the force of the charge was very ſenſibly diminiſhed by the addition of the globule of water, and the larger the quantity of water was that was thus confined, the leſs was the effect of the charge; neither the recoil of the piſtol, nor the penetration of the bullet, being near equal to what they were when the given quantity of powder was fired without the water; and the report of the exploſion appeared to be leſſened in a ſtill greater proportion than the recoil or penetration.

Concluding that this diminution of the force of the charge aroſe from the burſting of the little bladder, and the diſperſion of the water among the powder before it was all inflamed, by which a great part of it was prevented from taking fire, I repeated the experiments with highly rectified ſpirits of wine inſtead of water; but the reſult was nearly the ſame as before: the force of the charge was conſtantly and very ſenſibly diminiſhed. I afterwards made uſe of etherial oil of turpentine, and then of ſmall quantities of quickſilver; but ſtill with no

better ſucceſs. Every thing I mixed with the powder, inſtead of increaſing, ſerved to leſſen the force of the charge.

Theſe trials were all made ſeveral months before I began the courſe of my experiments upon gun-powder, which I have already given an account of; and though they were altogether unſucceſsful, yet I reſumed the inquiry at that time, and made ſeveral new experiments, with a view to find out ſomething that ſhould be ſtronger than powder, or which, when mixed with it, ſhould increaſe its force.

It is well known, that the elaſtic force of quickſilver converted into vapour is very great; this ſubſtance I made uſe of in my former trials, as I have juſt obſerved, but without ſucceſs. I thought, however, that the failure of that attempt might poſſibly be owing to the quickſilver being too much in a body, by which means the fire could not act upon it to the greateſt advantage; but that if it could be divided into exceeding ſmall particles, and ſo ordered that each particle might be completely ſurrounded by, and expoſed to, the action of the flame of the powder, it would be very ſoon heated, and poſſibly might be converted into an elaſtic ſteam or vapour, before the bullet could be ſenſibly removed from its place. To determine this point I mixed 20 grains of æthiops mineral very intimately with 145 grains of powder, and charging the piece with this compound, it was loaded with a fit bullet and fired; but the force of the charge was leſs than that which the powder alone would have exerted, as appears by comparing the 76th and 77th experiments with the 79th.

Common *pulvis fulminans* is made of one part of ſulphur, two parts of ſalt of tartar, and three parts of nitre; and if we may judge by the report of the exploſion, the elaſtic force of this compound is conſiderably greater than that of gun-powder.

I was

I was willing to ſee the effect of mixing ſalt of tartar with gun-powder, and accordingly having provided ſome of this alkaline ſalt in its pureſt ſtate, thoroughly dry, and in a fine powder, I mixed 20 grains of it with 145 grains of gun-powder; and upon diſcharging a bullet with the mixture, I found that the alkaline ſalt had conſiderably leſſened the force of the powder. See experiment N° 78.

I next made uſe of *ſal ammoniacum*. That ſalt has been found to produce a very large quantity of elaſtic air, or vapour, when expoſed to heat under certain circumſtances; but when 20 grains of it were mixed with a charge of gun-powder, inſtead of adding to its force, it diminiſhed it very ſenſibly. See the 80th experiment.

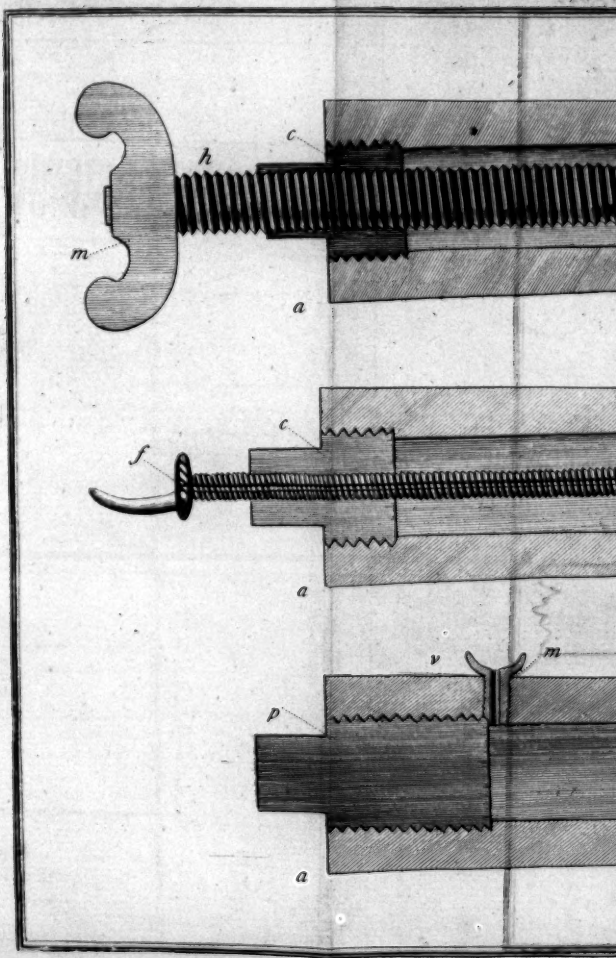
Moſt, if not all, the metals, are thought to produce large quantities of air when they are diſſolved in proper *menſtrua*, and particularly braſs, when it is diſſolved in ſpirit of nitre. Deſirous of ſeeing if this could be done by the flame, or acid vapour of fired powder, I mixed 20 grains of braſs in a very fine powder, commonly called braſs duſt (being the ſmall particles of this metal that fly off from the wheel in ſharpening pins), with 145 grains of powder, and with this compound and a fit bullet I loaded my barrel and diſcharged it; but the experiment (N° 81.) ſhewed, that the force of the powder was not increaſed by the addition of the braſs duſt, but the contrary.

It ſeems probable, however, that neither braſs duſt nor æthiops mineral are of themſelves capable of diminiſhing the force of gun-powder in any conſiderable degree, otherwiſe than, by filling up the interſtices between the grains, and obſtructing the paſſage of the flame, and ſo impeding the progreſs of the inflammation. And hence it appears, how earthy particles and

impurities of all kinds are ſo very detrimental to gun-powder. It is not that they deſtroy or alter the properties of any of the bodies of which the powder is compoſed, but ſimply, that by obſtructing the progreſs of the inflammation, they leſſen its force, and render it of little or no value. Too much care, therefore, cannot be taken in manufacturing powder to free the materials from all heterogeneous matter.

Of an attempt to ſhoot flame inſtead of bullets.

Having often obſerved paper and other light bodies to come out of great guns and ſmall arms inflamed, I was led to try if other inflammable bodies might not be ſet on fire in like manner, and particularly inflammable fluids; and I thought if this could be effected, it might be poſſible to project ſuch ignited bodies by the force of the exploſion, and by that means communicate the fire to other bodies at ſome conſiderable diſtance; but in this attempt I failed totally. I never could ſet dry tow on fire at the diſtance of five yards from the muzzle of my barrel. I repeatedly diſcharged large wads of tow and paper, thoroughly ſoaked in the moſt inflammable fluids, ſuch as *alkohol*, *ætherial ſpirit of turpentine*, *balſam of ſulphur*, &c.; but none of them were ever ſet on fire by the exploſion. Sometimes I diſcharged three or four ſpoonfuls of the inflammable fluid, by interpoſing a very thin wad of cork over the powder, and another over the fluid; but ſtill with no better ſucceſs. The fluid was projected againſt the wall as before, and left a mark where it hit; but it never could be made to take fire; ſo I gave up the attempt. If it had ſucceeded, probably it would have turned out one of the moſt important diſcoveries in the art of war that have been made ſince the invention of gun-powder.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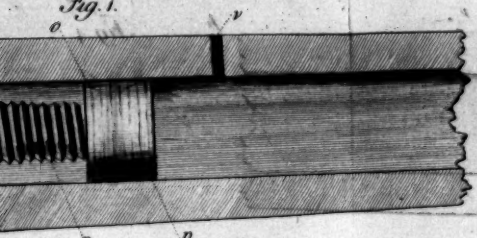

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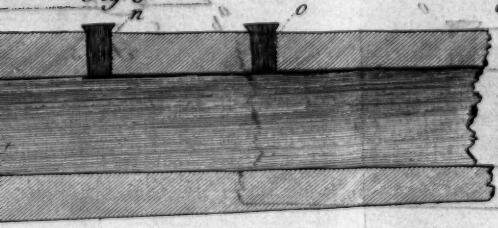

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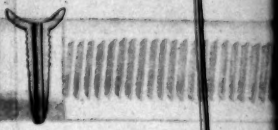

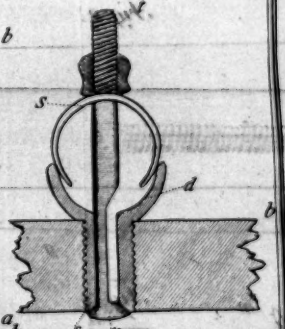

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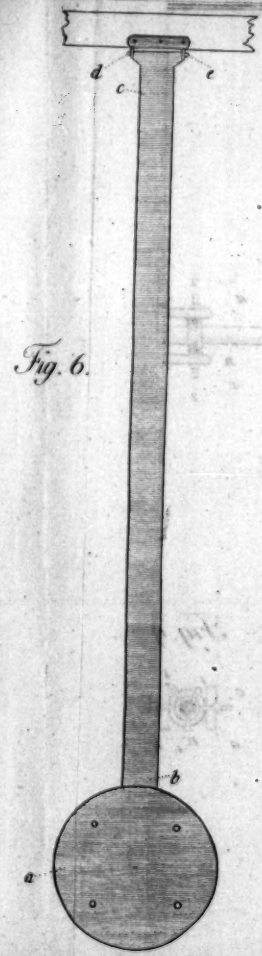

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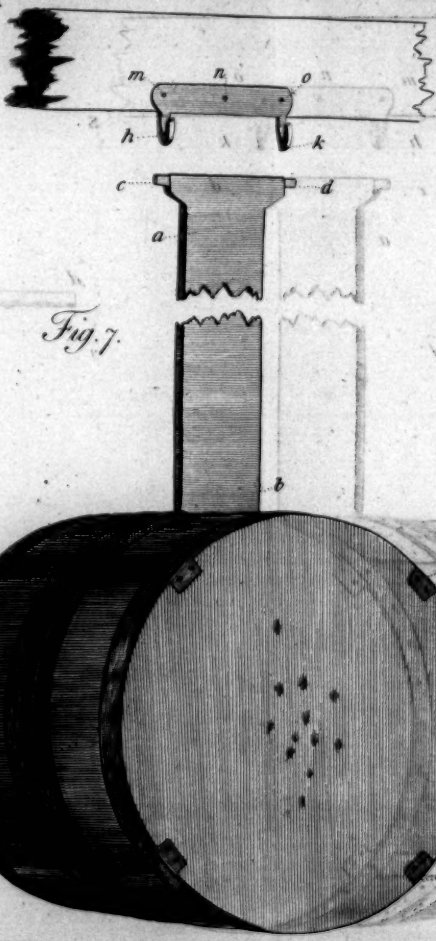

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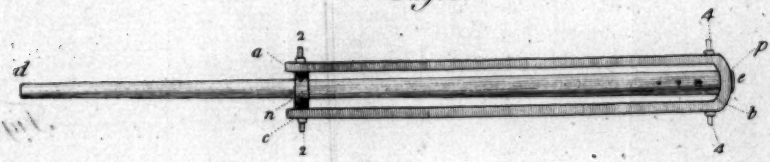

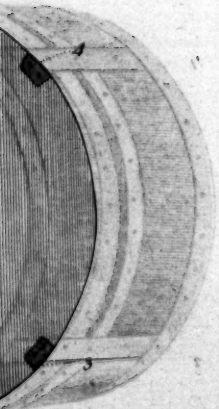

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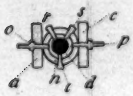

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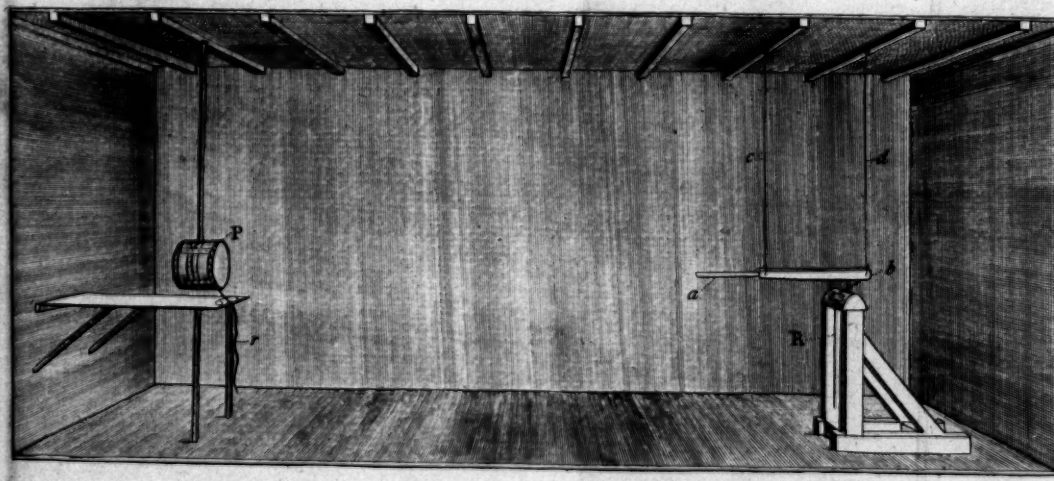

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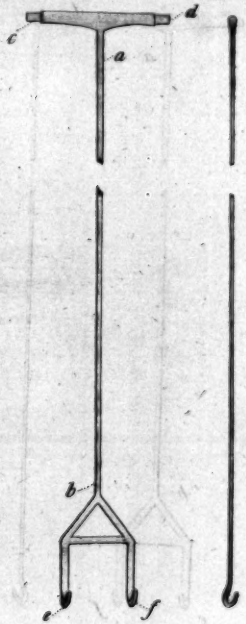

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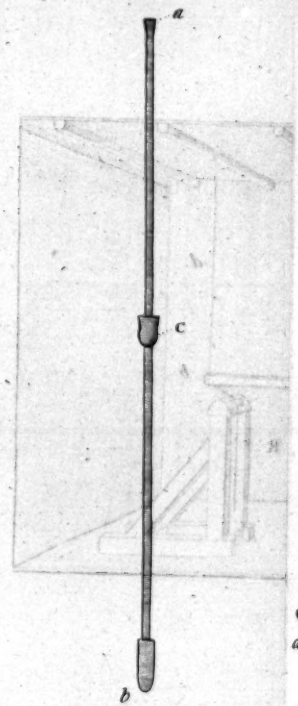

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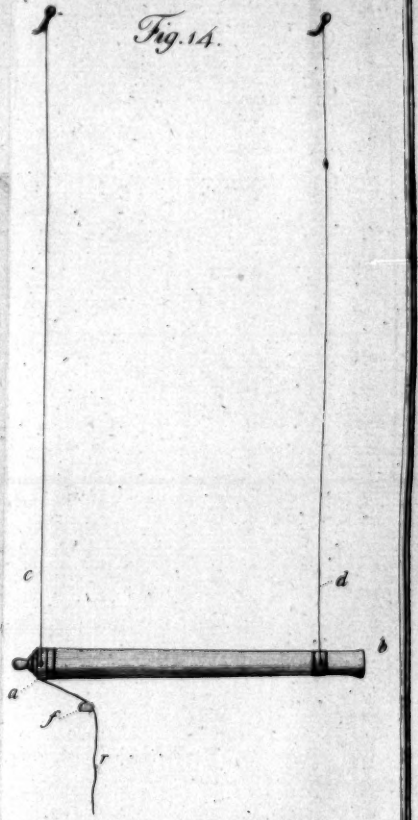

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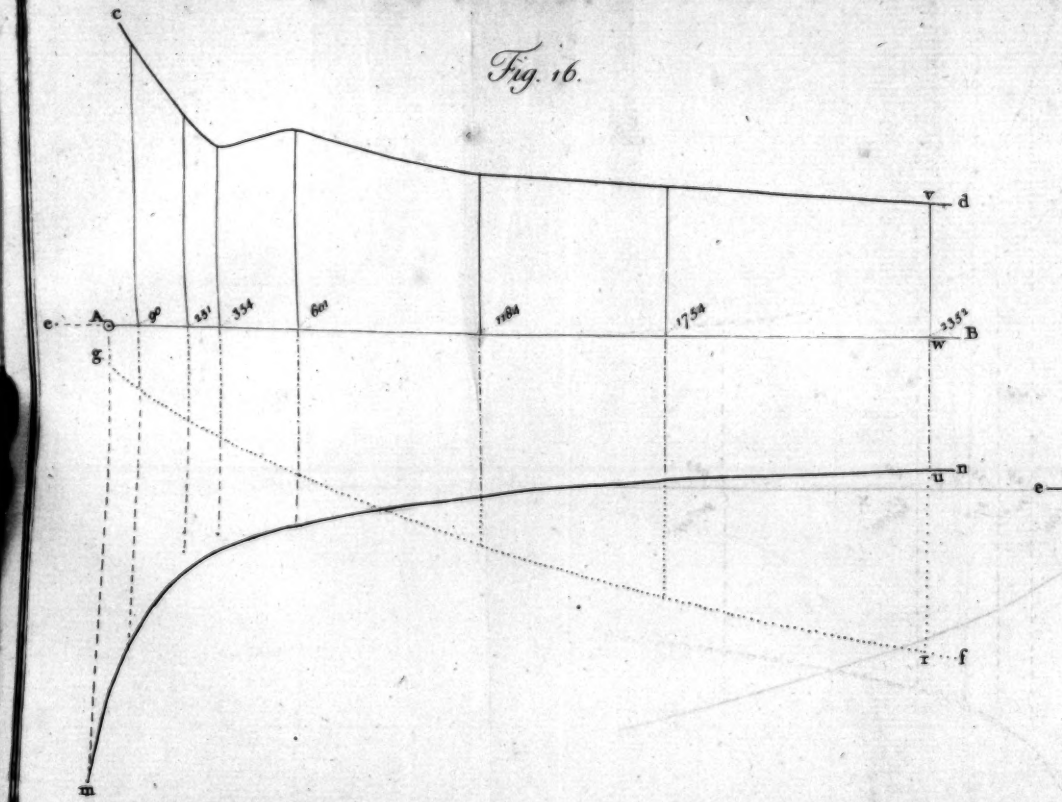

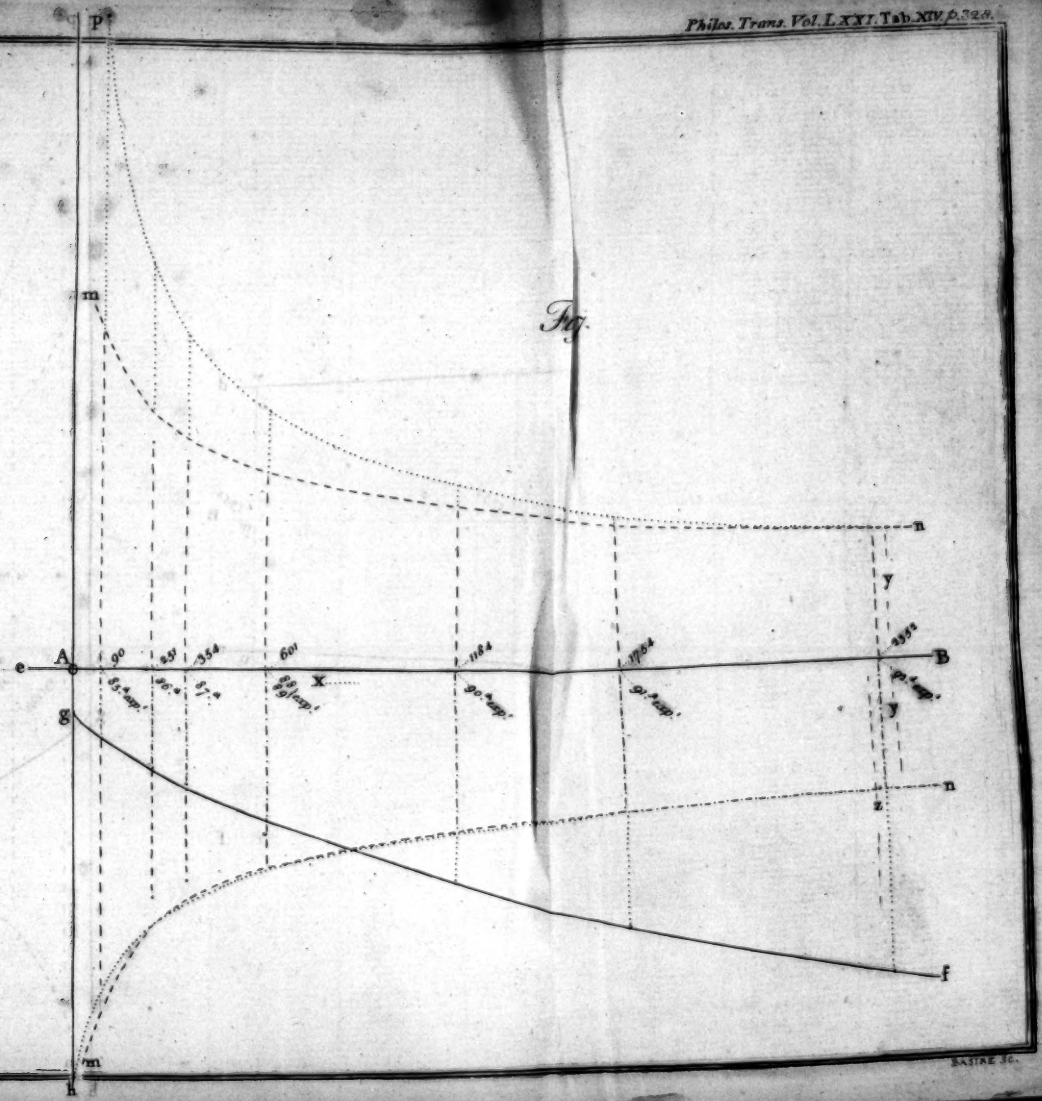

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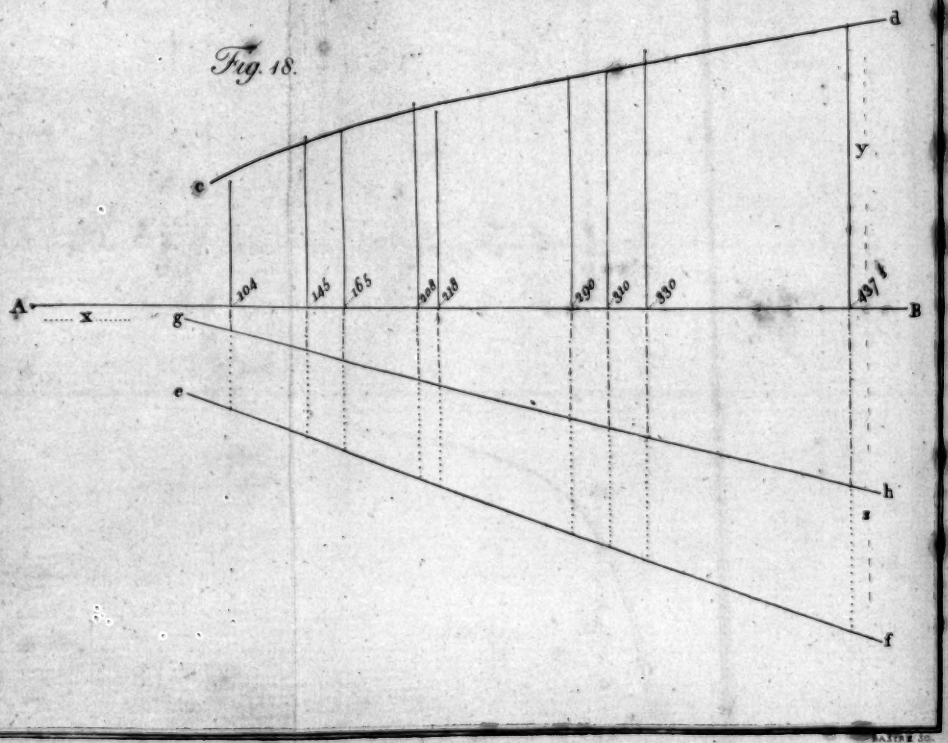


XVI. *Account of a luminous Appearance in the Heavens. By Mr.* Tiberius Cavallo, *F. R. S. in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 5, 1781.

I TAKE the liberty to ſend you an account of a luminous appearance obſerved laſt night in the heavens, which ſeems to be very ſingular in its nature, and quite diſtinct from the *aurora borealis*.

At about half an hour paſt nine yeſterday evening, being the 27th of March, a white light began to be ſeen in the ſky, which became gradually more and more denſe till ten o'clock, at which time it formed a compleat luminous arch from eaſt to weſt. Of this I have been informed by others; but at a quarter paſt ten I went out of the houſe and obſerved it myſelf. At that time it appeared to be an arch of about ſeven or eight degrees in breadth extended from eaſt to weſt, or, as ſome of my friends imagined, in the direction of eaſt by north to weſt by ſouth. Its weſtern part quite reached the horizon; but the eaſtern part of the arch ſeemed to begin at about 50° or 60° above the horizon. It did not paſs through the zenith but at about 8° or 10° ſouthward of it, and it was nearly perpendicular to the horizon.

The whiteneſs of this arch was much denſer than that any *aurora borealis* I ever obſerved, though it did not caſt ſo much light upon the terreſtrial objects. Towards the middle it was ſo denſe, that the ſtars over which it paſſed were

eclipſed; but the ſides of this luminous arch were more faint and tranſparent.

The incloſed drawing ſhews its appearance at about half an hour paſt ten o'clock. At about three quarters paſt ten it began to loſe its brightneſs, firſt at A, and then vaniſhed gradually, ſo that at eleven o'clock none of it could be perceived. As ſoon as any part of this arch loſt its denſe whiteneſs, the ſtars appeared through it quite diſtinct, ſo that it could not be a cloud. The light alſo ſeemed to vaniſh without change of place; for it did not appear to be diſperſed through the ſky, or to be driven in any direction.

This extraordinary appearance to me ſeemed quite diſtinct from the *aurora borealis* for the following reaſons, *viz.* becauſe it eclipſed the ſtars over which it paſſed; becauſe its light, or rather its white appearance, was ſtationary and not lambent; and becauſe its direction was from eaſt to weſt.

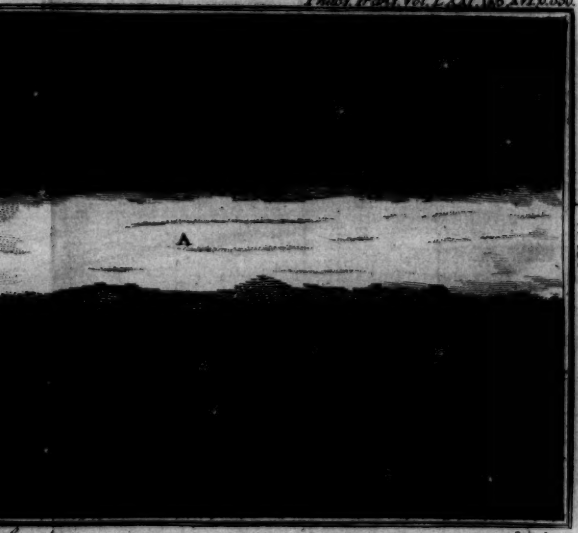
The atmoſphere was in other reſpects very ſerene, the ſtars ſhining very bright, and no cloud appearing. The northern light was exceedingly faint, and very low about the northern point of the horizon. The wind was nearly north-eaſt, and it could be juſt perceived in the ſtreets.

I am, &c,

North
East
South

North

Phil. Trans. Vol. L. XXI. Tab. XVI. p. 330.



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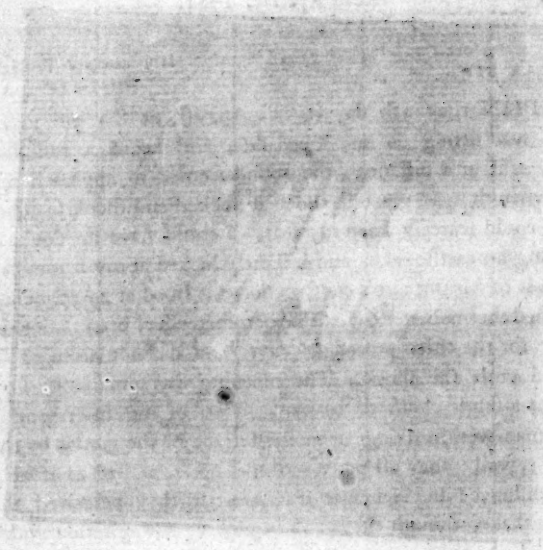


XVII. *Account of an Earthquake at* Hafodunos *near* Denbigh. *By* John Lloyd, *Eſq. F. R. S. in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 5, 1781.

Hafodunos near Denbigh,
Dec. 31, 1781.

DEAR SIR,

UPON the 29th day of laſt Auguſt, at 8 h. 37′ 30″, I was ſitting on my * bed-ſide, and heard a rumbling noiſe, as if at a diſtance: the ſound ſeemed to approach me, and when it was greateſt the bed rocked and ſhook ſo much that I could ſcarcely keep my ſeat. I could have no doubt of its being an earthquake, and inſtantly looked at my barometer, which is of Mr. DE LUC's conſtruction; it ſtood at 29.57 inches. Attached thermometer 65°. The barometer had been ſtationary nearly for the three preceding days, and did not ſeem to be affected with the ſhock. The morning was remarkably fine, and not a ſingle cloud to be ſeen. Two of my ſiſters and a gentleman were walking upon the terrace in the garden by the ſide of a wall: they all perceived the noiſe, at firſt as if at a great diſtance; but when it was greateſt they perceived the wall to ſhake, though they did not obſerve any agitation under their feet. As they were walking, and obſerved the ſpot when

* This houſe is built upon the ſide of a rock; and my bed-chamber, though up two pair of ſtairs, is on a ground-floor, the floor is not more than one foot from the ſolid rock in my bed-chamber.

they

they firſt heard the ſound, and the ſpot they came to when it ceaſed, I was enabled to aſcertain its duration pretty exactly, and find it to have been from fifteen to eighteen ſeconds. Its courſe was nearly from ſouth-eaſt to north-weſt. Some other perſons in our houſe perceived a double ſhock; and this has been obſerved by many who felt it in other places. It was felt at Flint by Mrs. SEAMAN and her daughter, who obſerved the cups rattled upon the ſaucers as they ſat at breakfaſt. Mr. PENNANT's family, at Downing, fancied that an empty waggon was coming into the back-court, which is paved. It was ſtrongly felt at Llonrwſt by the whole town, and part of a ſtone wall was flung down. At Carnarvon (which is in the ſame parallel of latitude as this place, 53° 10′) the ſhock was very ſlight. It was perceived in many places about Conway; but not at all by any one in town. Sir HUGH WILLIAMS felt it very ſtrongly at his houſe near Beaumaris. At our friend Mr. DAVIES's, in that town, a door clapped backwards and forwards ſeveral times; and at Lord BULKELEY's ſeat, Boron-hill, the family were much alarmed, it was ſo violent. It was ſtrongly felt at Holyhead, and at an eminent ſolicitor's in the iſland of Angleſey, the deſks before ſeveral clerks in his office ſhook ſo that they could not write. It was ſtrongly perceived at Mr. FITZMAURICE's, at Lleweny-hall in the Vale of Clwyd, and in ſeveral other places in that Vale. All the peninſula in Carnarvonſhire called Llun, ſurrounded by St. George's Channel, was ſhook very much. There have been two ſhocks ſince this I have been deſcribing. Mr. PENNANT felt one; but I was not ſenſible of either. The times it was felt at differ very much on account of the variations in the ſeveral dials from whence the clocks are regulated; but I am very exact as to my own time, having the day preceding the earthquake, and that very day,

aſcertained

aſcertained my time by equal altitudes, taken with one of Mr. BIRD's aſtronomical quadrants of one foot radius. As every phenomenon of this kind is intereſting, you may, perhaps, wiſh to communicate this account to the Royal Society; which you are welcome to do, if you think it worth the attention of ſo illuſtrious a body.

I have the honour to be, &c.

XVIII. *On the Heat of the Water in the Gulf-ſtream.* By Charles Blagden, *M. D. Phyſician to the Army, F. R. S.*

Read April 26, 1781.

ONE of the moſt remarkable facts obſerved in navigating the ocean, is that conſtant and rapid current which ſits along the coaſt of North America to the northward and eaſtward, and is commonly known to ſeamen by the name of the Gulf-ſtream. It ſeems juſtly attributed to the effect of the trade-winds, which blowing from the eaſtern quarter into the great Gulf of Mexico, cauſe there an accumulation of the water above the common level of the ſea; in conſequence of which, it is conſtantly running out by the channel where it finds leaſt reſiſtance, that is, through the Gulf of Florida, with ſuch force as to continue a diſtinct ſtream to a very great diſtance. Since all ſhips going from Europe to any of the ſouthern provinces of North America muſt croſs this current, and are materially affected by it in their courſe, every circumſtance of its motion becomes an object highly intereſting to the ſeaman, as well as of great curioſity to the philoſopher. An obſervation which occurred to me on the ſpot ſuggeſts a new method of inveſtigating a matter that appears ſo worthy of attention.

During a voyage to America in the ſpring of the year 1776, I uſed frequently to examine the heat of ſea-water newly drawn, in order to compare it with that of the air. We made our paſſage

far to the southward. In this situation, the greatest heat of the water which I observed was such as raised the quicksilver in FAHRENHEIT's thermometer to 77°½. This happened twice; the first time on the 10th of April, in latitude 21° 10′ N. and longitude, by our reckoning, 52° W; and the second time, three days afterwards, in latitude 22° 7′ and longitude 55°; but in general the heat of the sea near the tropic of Cancer about the middle of April was from 76° to 77°.

The rendezvous appointed for the fleet being off Cape Fear, our course, on approaching the American coast, became north-westward. On the 23d * of April the heat of the sea was 74°, our latitude at noon 28° 7′ N. Next day the heat was only 71°; we were then in latitude 29° 12′; the heat of the water, therefore, was now lessening very fast in proportion to the change of latitude. The 25th our latitude was 31° 3′; but though we had thus gone almost 2° farther to the northward, the heat of the sea was this day rather increased, it being 72° in the morning, and 72°½ in the evening. Next day, the 26th of April, at half after eight in the morning, I again plunged the thermometer into sea-water, and was greatly surprised to see the quicksilver rise to 78°, higher than I had ever observed it, even within the tropic. As the difference was too great to be imputed to any accidental variation, I immediately conceived that we must have come into the Gulf-stream, the water of which still retained great part of the heat that it had acquired in the torrid zone. This idea was confirmed by the subsequent regular and quick diminution of the heat: the ship's run for a quarter of an hour had lessened it 2°; the thermometer,

* From the difference between civil and nautical time, it becomes necessary to observe, that the former is always meant in this paper.

at three quarters after eight, being raiſed by ſea-water freſh drawn only to 76°; by nine the heat was reduced to 73°, and in a quarter of an hour more, to 71° nearly: all this time the wind blew freſh, and we were going ſeven knots an hour on a north-weſtern courſe. The water now began to loſe the fine tranſparent blue colour of the Ocean, and to aſſume ſomething of a greeniſh olive tinge, a well-known indication of ſoundings. Accordingly, between four and five in the afternoon ground was ſtruck with the lead at the depth of eighty fathom, the heat of the ſea being then reduced to 69°. In the courſe of the following night and next day, as we came into ſhallower water and nearer the land, the temperature of the ſea gradually ſunk to 65°, which was nearly that of the air at the time.

Unfortunately bad weather on the 26th prevented us from taking an obſervation of the ſun; but on the 27th, though it was then cloudy at noon, we calculated the latitude from two altitudes, and found it to be 33° 26′ N. The difference of this latitude from that which we had obſerved on the 25th, being 2° 23′, was ſo much greater than could be deduced from the ſhip's run marked in the log-book, as to convince the ſeamen that we had been ſet many miles to the northward by the current.

On the 25th at noon, the longitude by our reckoning was 74° W. and I believe the computation to have been pretty juſt; but the ſoundings, together with the latitude, will determine the ſpot where theſe obſervations were made better than any reckoning from the eaſtward. The ſhip's run on the 26th, from nine in the forenoon to four in the afternoon, was about ten leagues on a north-weſt by north courſe; ſoon afterwards

we

we hove-to in order to ſound, and, finding bottom, we went very ſlowly all night, and till noon the next day.

From theſe obſervations, I think, it may be concluded, that the Gulf-ſtream, about the 33d degree of north latitude, and the 76th degree of longitude weſt of Greenwich, is, in the month of April, at leaſt ſix degrees hotter than the water of the ſea through which it runs. As the heat of the ſea-water evidently began to increaſe in the evening of the 25th, and as the obſervations ſhew that we were getting out of the current when I firſt tried the heat in the morning of the 26th, it is moſt probable, that the ſhip's run during the night is nearly the breadth of the ſtream meaſured obliquely acroſs; that, as it blew a freſh breeze, could not be much leſs than twenty-five leagues in fifteen hours, the diſtance of time between the two obſervations of the heat, and hence the breadth of the ſtream may be eſtimated at twenty leagues. The breadth of the Gulf of Florida, which evidently bounds the ſtream at its origin, appears by the charts to be two or three miles leſs than this, excluding the rocks and ſand-banks which ſurround the Bahama Iſlands, and the ſhallow water that extends to a conſiderable diſtance from the Coaſt of Florida; and the correſpondence of theſe meaſures is very remarkable, ſince the ſtream, from well-known principles of hydraulics, muſt gradually become wider as it gets to a greater diſtance from the channel by which it iſſues.

If the heat of the Gulf of Mexico was known, many curious calculations might be formed by comparing it with that of the current. The mean heat of Spaniſh-town and Kingſton in Jamaica ſeems not to exceed 81°*; that of St. Domingo on

* Hiſtory of Jamaica, London, 1774, vol. III. p. 652, 653. The different obſervations of the heat recorded in that work do not agree together; but thoſe adopted here are taken from that ſeries which appeared to me the moſt correct.

the

the sea-coast may be estimated at the same from Monsr. GODIN'S observations *; but as the coast of the continent which bounds the gulf to the westward and southward is probably warmer, perhaps a degree or two may be allowed for the mean temperature of the climate over the whole bay: let it be stated at 82° or 83°. Now there seems to be great probability in the supposition that the sea, at a certain comparatively small distance below its surface, agrees in heat pretty nearly with the average temperature of the air during the whole year in that part; and hence it may be conjectured, that the general heat of the water, as it issues out of the bay to form the stream, is about 82° †, the small variations of temperature on the surface not being sufficient to affect materially that of the general mass. At the tropic of Cancer I found the heat to be 77°; the stream, therefore, in its whole course from the gulf of Florida, may be supposed to have been constantly running through water from 4° to 6° colder than itself, and yet it had lost only 4° of

* Monsr. GODIN'S experiments upon the pendulum were made at the Petit Goave. They continued from the 24th of August to the 4th of September, and the average heat during that time was such as is indicated by 25° of Monsr. DE REAUMUR'S thermometer (see Mem. Acad. Scienc. 1735, p. 517.). According to Monsr. DE LUC'S calculation (see Modifications de l'Atmospere, vol. I. p. 378.) the 25th degree of Monsr. DE REAUMUR'S *true* thermometer answers to about the 85th of FAHRENHEIT'S; but the average heat in Jamaica during the months of August and September is also 85°; hence we may conclude, that the mean heat for the whole year is nearly the same on the sea-coasts in both islands.

† The lowest calculation of the mean temperature of the gulf is preferred on this occasion, because of the constant influx of new water from the Atlantic Ocean produced by the trade-winds; which water not having been near any land must, I think, be sensibly cooler than that which has remained some time inclosed in the bay. On this subject the observations made by ALEXANDER DALRYMPLE, Esq. relative to the heat of the sea near the Coast of Guinea, ought to be consulted (see Phil. Trans. vol. LXVIII. p. 394, &c.).

heat,

heat, though the ſurrounding water where I obſerved it was 10° below the ſuppoſed original temperature of the water which forms the current. From this ſmall diminution of the heat, in a diſtance, probably of 300 miles, ſome idea may be acquired of the vaſt body of fluid which ſets out of the gulf of Mexico, and of the great velocity of its motion. Numerous obſervations on the temperature of this ſtream, in every part of it, and at different ſeaſons of the year, compared with the heat of the water in the ſurrounding ſeas, both within and without the tropic, would, I apprehend, be the beſt means of aſcertaining its nature, and determining every material circumſtance of its movement, eſpecially if the effect of the current in puſhing ſhips to the northward is carefully attended to, at the ſame time with the obſervations upon its heat.

On the 25th of September 1777, as the ſhips which had tranſported Sir WILLIAM HOWE's army up Cheſapeak Bay were returning toward the Delaware, with the ſick and ſtores, they were overtaken, between Cape Charles and Cape Hinlopen, by a violent gale of wind, which, after ſome variation, fixed ultimately at N. N. E. and continued five days without intermiſſion. It blew ſo hard that we were conſtantly loſing ground, and driving to the ſouthward: we alſo purpoſely made ſome *caſting* to keep clear of the dangerous ſhoals which lie off Cape Hatteras.

The 28th at noon our latitude was 36° 40′ N. and the heat of the ſea all day about 65°. On the 29th our latitude was 36° 2′; we had, therefore, in the courſe of theſe twenty-four hours, been driven by the wind 38 nautical miles to the ſouthward:

ward: the temperature of the ſea continued nearly at 65°. Next day, the 30th, our latitude at noon was 35° 44′, only 18 miles farther to the ſouthward, though in the opinion of the ſeamen aboard, as well as my own, it had blown at leaſt as hard on this as any of the preceding days, and we had not been able to carry more ſail; conſequently it may be concluded, that ſome current had ſet the ſhip 20 miles to the northward. To know whether this was the Gulf-ſtream, let us conſult the thermometer. At half after nine in the forenoon of this day the heat of this water was 76°, no leſs than *eleven* degrees above the temperature of the ſea before we came into the current!

Towards evening the wind fell, and we ſtood N.W. by N. cloſe-hauled. As the ſea ſtill ran very high, and the ſhip ſcarcely went above two knots an hour, we did not make leſs than three points of lee-way on this tack; the courſe we made good, therefore was W.N.W. which, on the diſtance run by noon next day, gave us about ſixteen miles of *northing*; but that day, the 1ſt of October, our latitude was 36° 22′, 38 miles farther to the north than we had been the day before; the difference, 22 miles, muſt be attributed to the Gulf-ſtream. This, however, is only part of the effect which the current would have produced upon the ſhip if we had continued in it the whole four and twenty hours; for, though we were ſtill in the ſtream at five in the afternoon of the 30th, as appeared by the heat of the water being then above 75°, and at eight in the evening the heat being ſtill 74°, yet by ſeven next morning we were certainly got clear of it, the heat of the ſea being then reduced to its former ſtandard of 65°. On this occaſion, therefore, we did not croſs the ſtream, but having fallen-in with it obliquely

obliquely on the weſtern ſide, we puſhed out again on the ſame ſide as ſoon as the gale abated.

Theſe obſervations having been made three degrees to the northward of my former ones, it is curious to obſerve, that the heat of the Gulf-ſtream was about 2° leſs. The ſeaſons of the year, indeed, were very different; but, perhaps, under ſuch circumſtances that their effects were nearly balanced. In the latter obſervations the meridian altitude of the ſun was leſs; but then a hot ſummer preceded them: whereas in the former, though the ſun's power was become very great, yet the winter had been paſt but a ſhort time. Calculating upon this proportion we may be led to ſuſpect, that about the 27th degree of latitude, which is as ſoon as the ſtream has got clear of the gulf of Florida, it begins ſenſibly to loſe its heat from 82°, the ſuppoſed temperature of the gulf of Mexico, and continues to loſe it at the rate of about 2° of FAHRENHEIT's ſcale to every 3° of latitude, with ſome variation, probably as the ſurrounding ſea, and the air, are warmer or colder at different ſeaſons of the year.

The preceding facts had made me very deſirous of obſerving the heat of the Gulf-ſtream on my paſſage homeward; but a violent gale of wind, which came on two days after we had ſailed from Sandy Hook, diſabled every perſon aboard, who knew how to handle a thermometer, from keeping the deck. The maſter of the ſhip, however, an intelligent man, to whom I had communicated my views, aſſured me, that on the ſecond day of the gale the water felt to him remarkably warm; we were then near the 70th degree of weſt longitude. This agrees very well with the common remark of ſeamen, who alledge, that they are frequently ſenſible of the Gulf-ſtream off Nan-

tucket ſhoals, a diſtance of more than 1000 miles from the gulf of Florida! According to the calculation I have before adopted of a loſs of two degrees of heat for every 3° of latitude, the temperature of the Gulf-ſtream here would be nearly 73°; the difference of which from 59°, the heat that I obſerved in the ſea-water both before and after the gale, might eaſily be perceived by the maſter of the veſſel. This was in the winter ſeaſon, at the end of December.

An opinion prevails among ſeamen, that there is ſomething peculiar in the weather about the Gulf-ſtream. As far as I could judge, the heat of the air was conſiderably increaſed by it, as might be expected; but whether to a degree or extent ſufficient for producing any material changes in the atmoſphere muſt be determined by future obſervations.

Perhaps other currents may be found which, iſſuing from places warmer or colder than the ſurrounding ſea, differ from it in their temperature ſo much as to be diſcovered by the thermometer. Should there be many ſuch, this inſtrument will come to be ranked among the moſt valuable at ſea; as the difficulty of aſcertaining currents is well known to be one of the greateſt defects in the preſent art of navigation.

In the mean time, I hope the obſervations which have been here related are ſufficient to prove, that in croſſing the Gulf-ſtream very eſſential advantages may be derived from the uſe of the thermometer: for if the maſter of a ſhip, bound to any of the ſouthern provinces of North America, will be careful to try the heat of the ſea frequently, he muſt diſcover very accurately his entrance into the Gulf-ſtream, by the ſudden increaſe of the heat; and a continuance of the ſame experiments will ſhew him, with equal exactneſs, how long he remains in it. Hence he will always be able to make a proper allowance

for the number of miles that the ſhip is ſet to the northward, by multiplying the time into the velocity of the current. Though this velocity is hitherto very imperfectly known, for want of ſome method of determining how long the current acted upon the ſhips, yet all uncertainty ariſing from thence muſt ſoon ceaſe, as a few experiments upon the heat of the ſtream, compared with the ſhip's run checked by obſervations of the latitude, will aſcertain its motion with ſufficient preciſion. From differences in the wind, and perhaps other circumſtances, it is probable, that there may be ſome variations in the velocity of the current; and it will be curious to obſerve, whether theſe variations may not frequently be pointed out by a difference in its temperature; as the quicker the current moves, the leſs heat is likely to be loſt, and conſequently the hotter will the water be. In this obſervation, however, the ſeaſon of the year muſt always be conſidered; partly, becauſe it may, perhaps, in ſome degree affect the original temperature of the water in the gulf of Mexico; but principally, becauſe the actual heat of the ſtream muſt be greater or leſs in proportion as the tract of the ſea through which it has flown was warmer or colder. In winter, I ſhould ſuppoſe, that the heat of the ſtream itſelf would be rather leſs than in ſummer; but that the difference between it and the ſurrounding ſea would be much greater; and I can conceive that, in the middle of ſummer, though the ſtream had loſt very little of its original heat, yet the ſea might, in ſome parts, acquire ſo nearly the ſame temperature, as to render it ſcarcely poſſible to diſtinguiſh by the thermometer when a ſhip entered into the current.

Beſides the convenience of correcting a ſhip's courſe, by knowing how to make a proper allowance for the diſtance ſhe is ſet to the northward by the current, a method of determining

with certainty when ſhe enters into the Gulf-ſtream is attended with the further ineſtimable advantage of ſhewing her place upon the ocean in the moſt critical ſituation: for, as the current ſets along the coaſt of America at no great diſtance from ſoundings, the mariner, when he finds this ſudden increaſe of heat in the ſea, will be warned of his approach to the coaſt, and will thus have timely notice to take the neceſſary precautions for the ſecurity of his veſſel. As the courſe of the Gulf-ſtream comes more to be accurately known, from repeated obſervations of the heat and latitudes, this method of determining the ſhip's place will be proportionably more applicable to uſe. And it derives additional importance from the peculiar circumſtances of the American coaſt, which, from the mouth of the Delaware to the ſouthernmoſt point of Florida, is every where low, and beſet with frequent ſhoals, running out ſo far into the ſea that a veſſel may be aground in many places where the ſhore is not to be diſtinguiſhed even from the maſt-head. The gulf-ſtream, therefore, which has hitherto ſerved only to increaſe the perplexities of ſeamen, will now, if theſe obſervations are found to be juſt in practice, become one of the chief means of their preſervation upon that dangerous coaſt.

XIX. *Account of the Appearance of the Soil at opening a Well at Hanby in Lincolnſhire. In a Letter from Sir Henry C. Engleſield, Bart. F. R. and A. S. to Sir Joſeph Banks, Bart. P. R. S.*

Read May 3, 1781.

DEAR SIR,

THE appearance of the ſoil which fell under my own inſpection, on opening a well at Hanby, the ſeat of Sir C. BUCK, in Lenton parish, Lincolnſhire, being, as far as I can recollect, quite ſingular, I hope you will not think this account of it unworthy the attention of the Society.

The ſpot on which the well was ſunk is nearly on a level with Lincoln Heath, and of courſe high ground compared with the fen, which is diſtant from it above ſix miles. The ſoil was uniformly a blue clay, in parts rather inclining to a ſhaly ſtructure, and contained many caſts of tellinæ, a very little pyrites, and ſome few ſmall, but very elegant, belemnites. Theſe are all the uſual foſſils of clay; but what I think without example is, that through the whole maſs of clay were interſperſed nodules of pure chalk, evidently rounded by long attrition, and of all ſizes from that of a pea to a child's head.

They lay in no ſort of order that I could find. How deep this appearance might have continued I cannot determine, but no water having been found at the depth of thirty feet, the

trial was given up, as the expence would have exceeded the advantage proposed. A specimen of the chalk is herewith exhibited to the Society.

I must add, that in all the environs there is not the least trace of chalk in any form whatever that I could discover or hear of.

I am, &c.

XX. *Astronomical Observations made by* Nathaniel Pigott, *Esq. F. R. S. Foreign Member of the Academies of* Bruſſels *and* Caen, *and Correſpondent of the Academy of Sciences at* Paris. *Communicated by Sir* Henry C. Engleſield, *Bart. F. R. and A. S.*

Read May 3, 1781.

HAVING given in the Philoſophical Tranſactions, vol. LXVI. for the year 1776, and vol. LXVIII. part II. for 1778, an ample deſcription of the excellent aſtronomical inſtruments in my poſſeſſion, it will be needleſs to ſay any thing here on that ſubject, further than that the following obſervations were made with them.

During the ſummer of 1777, part of which I ſpent with Lady WIDDRINGTON, at her houſe named Wickhill, about a mile from Stow on the Would, Glouceſterſhire, I determined, by ſix obſervations of ♃'s ſatellites, compared to correſpondent ones made on the ſame days, Wickhill W. of Greenwich 1° 29′ 45″. It is proper to add, this is likewiſe the longitude of Stow, it being under the ſame meridian, or very nearly ſo, as Wickhill.

In 1778 and 1779 I obſerved in Glamorganſhire; and by thirty-five meridian obſervations of the ſun and ſtars, all agreeing within 12″ from the mean, I determined the latitude of my obſervatory at Frampton Houſe 51° 25′ 1″ N.

Frampton

Frampton-house lies between Cowbridge and Lantwit; about four miles south of the former, and one mile north of the latter, and about two miles from the Bristol channel; is nearly under the same meridian as Watchet, a market town in Somersetshire.

The rocks on the Welch coast, which run obliquely slanting into the Bristol channel, render the navigation so dangerous, that each year affords the horrid spectacle of ships wrecked; and here I am sorry to add, that the barbarous custom of plundering these unfortunate vessels still subsists in all its inhumanity; at the same time it would be injustice to the gentlemen of the country to pass under silence their repeated endeavours to check this enormity; but hitherto their efforts have not been attended with much success: it is due to humanity to make such bad practices public, in hopes of exciting an inquiry, which justice and the honour of the nation loudly call for.

The little that has been said suffices to shew the expediency of correct maps of this channel. The universal opinion of the country is, that to the Somersetshire opposite coast, about Watchet, Porlock, &c. the breadth of the channel is twenty or twenty-one miles. A single glance of the eye seems sufficient to contradict this notion; however, as upon inspecting my maps I found the distances set down not greatly different from what report had made them, we measured them geometrically, and the result gave the channel, not twenty, but little more than thirteen miles broad at the abovementioned places.

Upon the whole it is to be wished, that astronomical observations, sufficiently correct, were made on the Somersetshire side, which might be compared with those I have made on the opposite shore. It may possibly be found, that the towns on the

English

Engliſh coaſt are placed in the maps too much ſouth, and thoſe in Wales too much north; and hence, perhaps, the too great breadth given to the Briſtol channel. This is, at leaſt, the caſe with the town of Lantwit, which, as I have ſaid, is to the ſouthward of Frampton-houſe, the latitude of which is 51° 25′ 1″; nevertheleſs the beſt and moſt extenſive map[*] I have been able to procure gives the latitude of Lantwit 51° 29′ 40″, that is to ſay 4′ 39″ N. of Frampton-houſe. There may very poſſibly be particular charts of the Briſtol channel more exact; but it is not leſs true, that the common maps ought to be cleared of ſuch enormous errors.

I determined the difference of meridians between Frampton-houſe and Greenwich by comparing four immerſions and fourteen emerſions of ♃'s firſt and ſecond ſatellites to correſponding ones made in other obſervatories. The reſult is as follows:

App. time.	Immerſions.
′ ″	
14 11	By an obſerv. at Greenwich.
13 52	—— at Upſal.
13 54	—— at Paris.
14 6	—— at Oxford.
14 1	the mean.

App. time.	Emerſions.
′ ″	
13 43	By an obſerv. at Oxford.
14 2	—— at Greenwich.
13 50	—— N. Almanack.
13 45	—— at Greenwich.
13 46	—— at Oxford.
13 44	—— at Paris.
14 12	—— at Paris.
13 50	—— at Oxford.
14 11	—— at Berlin.
13 48	—— at Oxford.
13 50	—— at Greenwich.
13 59	—— at Oxford.
14 3	—— at Greenwich.
14 7	—— at Greenwich.
13 55	Mean of emerſions.
14 1	Mean of immerſions.
13 58	by a mean of the means

Frampton-houſe W. of Greenwich in time, or 3° 29′ 30″ by the equator.

[*] By JOHN ADAMS. The ſcale is to minutes of a degree.

Occultations of fixed ſtars obſerved at Frampton-houſe in 1777 and 1778.

App. time.			
			October 21, 1777.
18	16	20	Imm. ♂ Pollux; doubtful to 2″ or 3″.
			November 15.
6	19	3	Imm. 1ſt, ☽ Tauri into light part of ☽: doubtful to 3″.
6	54	22	Imm. 2d, ☽ Tauri.
7	8	42	Emerſ. 1ſt, ☽ Tauri: dark part of ☽.
			November 16.
10	59	27	Imm. ζ Tauri: very good.
			July 5, 1778.
9	2	15½	Imm. teleſcopic ſtar: inſtantaneous.
9	3	28½	Imm. *vm*: inſtantaneous.
10	27	8½	Emerſ. *vm*, from light limb of ☽: ſure to 1″ or 2″.

Declination of the needle.

In the beginning of 1778 the declination weſt of a magnetic needle of four inches made by Mr. DOLLOND, appeared to be 22° 11′.

XXI. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1780. *By* Thomas Barker, *Esquire.*

Read May 3, 1781.

		Barometer.			Thermometer.						Rain.	Mean rain.	
					In the House.			Abroad.				10 yrs.	44 yrs.
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.		71—80	45 yrs. 36—80
Jan.	Morn.	29,89	28,16	29,39	39	28	34	36½	15½	27	1,013	1,677	1,573
	Aftern.				40	29	35	43	22½	32			
Feb.	Morn.	30,06	28,72	29,49	44	33	37	41	22	31	1,572	1,871	1,378
	Aftern.				44½	34	38	46	30½	39			
Mar.	Morn.	29,94	28,99	29,47	51½	41½	46	29	30	40	1,175	1,355	1,315
	Aftern.				53½	43½	47½	61	45	51			
Apr.	Morn.	29,77	28,49	29,21	53	40	46	52	29½	38	2,727	1,314	1,405
	Aftern.				55	41½	46	60	38	48			
May	Morn.	29,83	28,80	29,48	67	51	55	65	40½	50½	1,201	2,081	1,610
	Aftern.				72	52½	57	80½	53	61			
June	Morn.	29,89	29,26	29,55	65½	52	57½	62½	43	53½	1,920	2,374	2,249
	Aftern.				71½	52½	59	80	51	64			
July	Morn.	29,87	29,21	29,61	69½	58½	63	65	51½	57½	1,566	2,507	2,516
	Aftern.				71½	59½	64	83	61	69½			
Aug	Morn.	29,81	29,43	29,65	68	61½	64	63	52	58	0,422	2,468	2,247
	Aftern.				72	63	66	81½	63	70			
Sept.	Morn.	29,79	28,76	29,38	70	53	61	61	43	52	3,427	3,142	2,016
	Aftern.				73½	53½	63	82	53½	65			
Oct.	Morn.	29,87	28,20	29,23	58½	46½	52	56	33½	44	3,280	2,975	2,158
	Aftern.				59	48	53	64	45½	53			
Nov.	Morn.	30,00	28,62	29,41	50	38½	43	48½	19½	36	1,461	2,372	1,943
	Aftern.				50½	38½	43½	48	31	40			
Dec.	Morn.	30,08	29,22	29,83	45	35	40	46½	26	35	0,534	1,889	1,740
	Aftern.				45½	35	40½	50	30½	38½			
											20,108	26,024	22,210

The three drieſt ſeaſons from one month to twelve are the ſame as in vol. LXI. except the following.

Months							
2				Feb. and Mar. 79	0,370	Jan. and Feb. 79	0,451
3	Jan.—Mar. 79	0,582		Dec. 42—Feb. 43	0,934	Jan.—Mar. 40	0,942
7						Oct. 39—Apr. 40	0,543
10				Oct. 40—July 41	10,174		
12	Sept. 40—Aug. 41	13,427					

The three wetteſt ſeaſons from one month to twelve are entirely different from thoſe in vol. LXI.

Months.							
1	Sept. 74.	8,000		Nov. 70	7,818	May 73	6,843
2	Aug. and Sept. 74	11,910		Oct. and Nov. 70	10,932	Aug. and Sept. 75	10,430
3	July—Sept. 74	15,137		July—Sept. 75	14,508	Oct.—Dec. 70	13,545
4	July—Oct. 75	17,988		June—Sept. 74	17,620	Oct. 70—Jan. 71	14,958
5	July—Nov. 75	21,558		May—Sept. 74	20,762	Aug.—Dec. 70	17,038
6	July—Dec. 75	22,654		April—Sept. 74	23,285	Jun.—Nov. 70	18,978
7	July 75—Jan. 76	25,165		Mar.—Sept. 74	25,013	July 63—Jan. 64	22,902
8	July 75—Feb. 76	28,360		Feb.—Sept. 74	26,959	May—Dec. 73	25,623
9	Jan.—Sept. 74	30,267		July 75—Mar. 76	29,878	May 73—Jan. 74	28,931
10	Dec. 73—Sept. 74	33,164		May 73—Feb. 74	30,877	July 75—April 76	30,765
11	Nov. 73—Sept. 74	36,769		May 73—Mar. 74	33,605	July 75—May 76	32,392
12	Oct. 73—Sept. 74	39,390		May 73—April 74	35,128	May 75—April 76	34,877

The year began with froſt, and was perhaps the ſevereſt winter ſince 1740, but there was not a great deal of ſnow, and in general it was calm. The froſt was not ſo ſteady as it was that winter, there being ſeveral breaks in it; but was very ſharp, and the ice was never intirely gone for nine or ten weeks together from December 22, till near the end of February, when it went away without wet, leaving the ground remarkably light and fine, and the weather grew mild, and continued ſo moſt part of March; but the coldneſs of the ground hindered the graſs from growing greatly till toward the end of the time. The ſeed-

time

time was fine and good, and the grain came up very well, but the first three weeks of April were cold, backening, and often frosty. Toward the end it was more showery, warmer and growing, and from that time the spring continued to come on, and there were so few N.E. winds that ships found a difficulty in getting down the channel, which is very unusual at that time of year, and all the spring from the end of February till toward the end of June was very windy, chiefly N.W. and S.W.

In the former part of summer there were at times very hot days; but the season was oftener cool; many little showers, which in some countries were so small there was want of rain and grass, here we did pretty well. The hay-time was fine, but the crop small. The harvest was exceedingly well got: the barley and oats good, and some of the wheat; but the late-sown was thin through the severe winter, and in several places the wheat was mildewed, which could not be by wet such a year as this; but by this means wheat became three times the price of barley, being 50 and 52 shillings a quarter, and barley 16 or 17 shillings. From the latter part of July to the beginning of September it was very dry, hot, and burning; much scorching sun, the ground very much burnt up, and great want of water; but the N.E. winds, which came at this time of year instead of the spring, were sometimes fresh and cool.

The beginning of September the rains began, and for above two months there was a good deal, with such fine and warm weather, that there was good grass, a pleasant autumn, and very few frosty mornings, and the ground, which before was so dry, did not get much dirty with it. The wheat seed-time was fine, and the weather mild till the middle of November, when

when a hard frost, with snow, made people think of a hard winter; but it grew mild again, was chiefly dark and cloudy, but little rain, and drying a good deal of December; remarkably calm, but near a week's frost about Christmas.

The sickly seasons, which began in August 1779, continued more or less all the year; and about the same time of the year increased again. There were great numbers of fevers and agues, especially in and near the fens, which were very obstinate, and did not yield to the usual medicines, but frequently returned again, and hung very long on the patients.

XXII. *Some Calculations of the Number of Accidents or Deaths which happen in consequence of Parturition; and of the Proportion of Male to Female Children, as well as of Twins, monstrous Productions, and Children that are dead-born; taken from the Midwifery Reports of the* Westminster General Dispensary: *with an Attempt to ascertain the Chance of Life at different Periods, from Infancy to Twenty-six Years of Age; and likewise the Proportion of Natives to the rest of the Inhabitants of* London. *In a Letter from* Robert Bland, *M. D. Physician-Man-Midwife to the* Westminster General Dispensary, *to* Samuel Foart Simmons, *M. D. F. R. S.*

Read May 10, 1781.

DEAR SIR,

THE great advantage of hospitals and other similar institutions, in improving and disseminating medical knowledge, is generally acknowledged, but there are other purposes they seem equally calculated to answer, which, though subordinate to the former, may yet deserve attention, as they may throw light upon, and perhaps finally determine, certain political questions, about which various opinions are at present entertained. Thus, though it is known that this city contains persons from various countries, and that a very small portion of its inhabitants are natives; yet the proportion which the latter bear to the aliens can at present only be guessed at.

But

But this queſtion might be reſolved, with a tolerable degree of accuracy, if to the regiſter of the names of the perſons admitted to the ſeveral charities were added the places of their birth. Again, the great mortality of the human ſpecies, particularly in infancy, and the ſmall chance a child has to attain to years of maturity, have been calculated from pariſh-regiſters, bills of mortality, &c. But I do not know, that an attempt has hitherto been made to aſcertain them, by noting the number of children a promiſcuous multitude of women had borne, together with the number they had been able to preſerve.

This, however, is what I have here done; and from it I have attempted to form a table, ſhewing the chance of life at different periods. I am far from pretending that by this mode clear and certain intelligence will be obtained; but in a matter of ſuch moment, I preſume, that any aſſiſtance will be acceptable. Dr. SMELLIE * has curſorily mentioned, for the encouragement of his pupils, the ſmall proportion of the unnatural and laborious births to the natural; but he did not carry his views farther, or point out the proportionate number of conſequent accidents, which might occur to retard or prevent the recovery of the woman, although this is not leſs neceſſary to be known than the former. With a view to theſe, and other uſeful purpoſes, the following regiſter has been kept of the moſt material circumſtances concerning the patients admitted to the midwifery-department of the Weſtminſter General Diſpenſary, from its firſt inſtitution, in the year 1774, to the preſent time; *viz.*

1. The ages of the ſeveral women.
2. The number of children they had borne.
3. The ſexes of the children,

* See SMELLIE's Midwifry, 8vo, p. 195.

4. The number of children they had been able to preſerve.

5. The place or country where they and their huſbands were born.

And after the delivery of the patient I have conſtantly noted,

1. The accidents that attended, or were the conſequences of parturition.

2. The ſexes of the children delivered.

3. The number of twins or triplets.

4. The number of the children that were deficient or monſtrous.

5. The number of the children that were dead-born: and, as the women were enjoined to return their letters as ſoon as they were able to go abroad, I farther intended to have added the proportion of the children who died under four or five weeks; but many of the women neglecting this duty, prevented my information under this head from being ſo compleat as I could have wiſhed. Of thoſe, however, who came, or of whom certain account could be obtained, the number is ſet down.

From the above mentioned regiſter the following tables and accounts have been compoſed; and as the greateſt care and exactneſs were uſed in recording the ſeveral circumſtances, the ſame punctuality has been obſerved in collecting and digeſting them. And that they might be kept as free from error as poſſible, tables for each year were firſt compoſed and compared together; but finding no material variation, I did not think it neceſſary to produce them in that form. My firſt intention was to have given the tables ſimply, and without any explanatory obſervations; but finding I could not introduce all the circumſtances I had noted in my regiſter, as was particularly the caſe with regard to the firſt table, and imagining that

in ſome places they were not perfectly intelligible, without ſome explanation, as for inſtance in the table of the chance of life at different periods, I have ventured to add ſuch occaſional remarks as I think will tend to illuſtrate the ſubject.

As my firſt view was to find the proportion of difficult labours, and of the accidents or deaths that happen in conſequence of child-birth, I ſhall begin with the following TABLE.

Of 1897 women delivered under the care of the Diſpenſary,

63 or 1 in 30 had unnatural labours: in

18 of theſe, or 1 in 105, the children preſented by their feet; in

36 or 1 in 52, the breech preſented; in

8 the arms preſented; and in

1 the funis. } 9*, or 1 in 210.

63

17 women, or 1 in 111, had laborious labours: in

+8 of theſe, or 1 in 236, the heads of the children were leſſened; in

4 a ſingle blade of a forceps was uſed; and in the remaining

5 in which the faces of the children were turned to the pubes, the delivery was at length accompliſhed by the pains.

80 17

80

* In all theſe nine caſes the children were turned.

+ Two of theſe women have ſince been delivered of full-ſized healthy children. A third bore a very ſmall and weakly child, who died in two or three days. A fourth

80

1 woman had convulſions about the ſeventh month of her pregnancy, and was delivered a month after of a dead child, and recovered.

1 woman had convulſions during labour; brought forth a live child, and recovered.

*9 women, or 1 in 210, had uterine hæmorrhage before and during labour.

Of theſe 1 died undelivered;
1 died a few hours, and
1 ten days, after delivery, and
6 recovered.

9

5 women had the puerperal fever, of whom four died. In one of theſe the placenta was undelivered, and continued ſo to her death.

2 women were ſeized with mania, but recovered in about three months. In

1 woman a ſuppuration took place, ſoon after labour, from the vagina into the bladder and rectum. This patient recovered, but the urine and ſtools continue to paſs through the wounds. Of

99

fourth was delivered of a ſeven-months child, without mutilating it, which died in its paſſage. The number of women, therefore, who from error in their conformation were incapable of bearing live children appears to be very inconſiderable. Of the remaining four I have not been able to get any intelligence.

* In theſe nine caſes only one child was ſaved.

99

1 woman the perinæum was lacerated to the ſphincter ani. A ſuture was attempted, but without effect; ſhe recovered, but is troubled with prolapſus uteri.

5 had large and painful ſwellings of the legs and thighs, but recovered.

105 therefore of theſe, or 1 in 18, had preternatural or laborious births, or ſuffered in conſequence of labour. Of this number of caſes 43, or 1 in 44, were attended with particular difficulty or danger; and 7 only, or 1 in 270, died. The remaining 62 were delivered and recovered with little more than the common aſſiſtance: and

1792 had natural labours, not attended with any particular accidents.

1897

Of two women the uterus was retroverted in the third or fourth month of their pregnancy; but in both the uterus was replaced, and the women went to their full time, and brought forth live children.

Beſides the accidents above enumerated, it ſeems right to obſerve, that many of the women were afflicted with ſevere after-pains, or had what is called the milk-fever; but as theſe complaints were generally relieved in three or four days, and did not ſeem to have any influence in retarding their recovery, or to affect their future healths, no notice is taken of them. Some women alſo had ſymptoms of incipient prolapſus uteri, who had not before been troubled with that complaint; but as

I ſeldom ſaw them above once or twice, I cannot give the reſult. But as few of them were able to indulge themſelves with reſt, or to comply with the rules neceſſary for their cure, it is to be feared, that in many of them the complaint would gradually increaſe; and that in a courſe of years, the uterus would make its appearance externally, when, finding it an impediment to their activity, they would apply and ſubmit to wear a peſſary, or to ſuch palliatives as in that ſtate can only be adminiſtered. Excepting this accident, and the fluor albus, to which many of them are ſubject after child-bearing, but which, I think, does not often materially affect their healths until late in life, I am inclined to believe, that the lower ſort of people recover more certainly after parturition than perſons in higher ſtations of life: at leaſt, they are leſs ſubject to the puerperal fever, which is ſo fatal, if not checked on its firſt attack; and which, if not cauſed, is certainly nouriſhed, and its malignancy increaſed, by great fires, cloſe rooms, warm ſeptic diet and coſtiveneſs. But the apartments of the poor are generally ſo crazy, that without opening doors or windows, to which they are ſufficiently averſe, the air pours in upon them from all ſides. To this circumſtance, added to their inability to keep great fires, or to indulge themſelves with animal food, and to the care that is taken very early to empty their bowels, I have been induced to attribute their ſo generally eſcaping this fatal diſeaſe; and by adopting rules in my private practice conſonant to this idea, I have the ſatisfaction to be able to ſay, that I have not ſeen the puerperal fever among my private patients for more than three years.

TABLE of the proportion of male to female children, of the number of twins, and of the children that were deficient or monſtrous, and of thoſe that were dead-born.

1897 women were delivered of 1923 children; 972 boys and 951 girls, or as 46 boys to 45 girls.

23 of the women, or 1 in 80, were delivered of twins, 16 of whom were boys and 30 girls.

1 woman was delivered of 3 girls.

Of the twins and triplets, therefore, the males were only half the number of the females.

8 of the children, or 1 in 241, were deficient or monſtrous.

Of theſe 1 was web-fingered;
1 had a hare-lip;
1 had a dropſical head and diſtorted ſpine;
1 a dropſical head;
in 1 a part of the palate;
and in 2 a conſiderable portion of the cranium * was wanting;
and 1 had two heads †, ſee fig. 1.

8

One

* One of theſe lived an hour after it was born.

† This was the child of ELIZABETH wife of —— BROMFIELD, Peruke-maker, Jees-court, Oxford-ſtreet. It had two heads and necks, four hands and arms, two ſpines, uniting at the ſacrum, and terminating in one pelvis, from whence the lower extremities proceeded ſingle: there was one navel-ſtring, and one male organ of generation. On opening the body there were found, two thoracic cavities, the right more compleat than the left: the heart alſo, and the lungs on the right ſide, were more perfect than thoſe on the left, which latter were very ſmall.

One woman was delivered of a twin *, ſee fig. 2.

84 of the children, or 1 in 23 of the whole number, were dead-born †. Of theſe, 49, or nearly five-eighths, were boys, and 35 were girls.

Of 1400 women who returned their letters, or of whom a certain account could be obtained, 85, or nearly 1 in 16, had

There were two ſtomachs, two ſets of inteſtines, which, at length uniting, terminated in one rectum and anus. There was but one urinary bladder. The drawing that accompanies this will give a more juſt idea of its external figure; and Dr. HUNTER, who diſſected it, will probably one day oblige the world with an exact anatomical deſcription of it.

* Of this ſingular production, to which I have not ventured to give a name, the following is the hiſtory and deſcription. The woman who produced it is about twenty-ſeven years of age; this was her firſt pregnancy. She was, after a labour, delivered of a female fœtus, and its placenta, in which nothing uncommon was obſerved; and although the uterus remained of an unuſual ſize, yet the pains not recommencing, there was no ſuſpicion entertained but that its bulk was occaſioned by coagulated blood. On the third day the pains became violent, and this monſter was born. Its ſhape was ſpherical, but ſomewhat flattened. It meaſured in its largeſt diameter eight inches, and weighed about eighteen ounces. It received its nouriſhment by an umbilical chord, to which was attached a portion of membranes, and although no placenta was found, it is probable it had a ſmall one, and that it was incloſed in its own involucrum. It was completely covered with a cuticula, and a little above the part, where the navel-ſtring terminated, there was a hairy ſcalp covering a bony prominence, ſomewhat reſembling the arch of the cranium. On diſſection it was found to be plentifully ſupplied with blood veſſels, proceeding from the navel-ſtring, and branching through every part of it. It had a ſmall brain and medulla ſpinalis continued into a bony theca, with nerves paſſing from thence through the foramina of the bones; but no reſemblance of any thoracic or abdominal viſcera. The reſt of its bulk was made up of fat.

† By dead-born children I mean thoſe that die after they have been perceived to move, that is, generally after four months. Abortions, or deaths before that period, may reaſonably be eſtimated at double this number; ſo that, perhaps, 1 child in 8 dies in the womb, or in the act of coming into the world.

buried

buried their children before the end of two months. Of this number 53, or 5 in 8, were boys, and 32 girls.

This ſingular circumſtance of there being a greater number of males than females among the ſtill-born children, and of a greater number of male children dying in infancy than of females, has been remarked by Dr. PRICE and other writers on calculations; and Dr. HAYGARTH * has ſhewn that at Cheſter more huſbands die in a given period than wives. This naturally ſuggeſts an enquiry, whether the lives of males are at all ages more precarious than thoſe of females.

To be enabled to aſſiſt in anſwering this queſtion, I have added the following article to my regiſter, *viz.* of the children that ſhall be living at the time the women apply for their letters, how many will be boys, and how many girls?

* Obſervations on the bills of mortality in Cheſter for the year 1772.

TABLE

TABLE of the ages at which women begin and ceaſe to be capable of bearing children, and of the intermediate periods at which they are moſt ſo.

Of 2102 * pregnant women

	Years of age.	
†36 or 1 in 58 were from -	15 to 19	85, or 1 in 25, from 15 to 20 inclusive.
49 or 1 in 43 were -	20	
578 or 5 in 19 were from -	21 to 25	1684, or four-fifths were from 21 to 35 inclusive.
699 nearly 1 in 3 were from	26 to 30	
407 nearly 1 in 5 were from	31 to 35	
291 or 3 in 22 were from -	36 to 40	
36 or 1 in 58 were from -	41 to 45	42, or 1 in 50, from 41 to 49.
6 or 1 in 350 were from	46 to 49	
2102		

* Although 2102 women, the number here mentioned, obtained letters, entitling them to the aſſiſtance of the midwives, 1897 only were delivered by them: the remainder either removed out of the bounds of the Diſpenſary, or, from ſome alteration in their circumſtances, were obliged to go to an hoſpital or workhouſe.

† 1 of theſe women was between 15 and 16 years of age.
1 between 16 and 17;
3 between 17 and 18;
10 between 18 and 19; and
21 between 19 and 20.

36

TABLES of the number of children borne by 1389 * women, with the number that were living at the time of their applying to the Diſpenſary.

Women.	No of children borne by each woman.	Total of children born.	Total of children living.	No of women who had preſerved their children.	No of children preſerved by each woman.	Total of children preſerved.
1	24	24	5	——	——	——
1	17	17	3	——	——	——
3	16	48	5	——	——	——
2	14	28	11	——	——	——
11	13	155	46	——	——	——
14	12	168	44	——	——	——
15	11	165	45	1	11	11
22	10	220	84	——	——	——
33	9	297	93	——	——	——
56	8	448	151	4	8	32
74	7	518	213	3	7	21
89	6	534	214	11	6	66
138	5	690	288	32	5	160
169	4	676	293	84	4	336
208	3	624	299	174	3	522
254	2	508	259	306	2	612
299	1	299	171	464	1	464
1389		†5419	2224	1079		2224
and 370 were in their firſt pregnancy,				and 310 had loſt all their childr.		
1389				1389		

* In order to account for the difference between the number of the women in theſe and the preceding tables, it is proper to mention, that this account was not begun until ſome months after the former one. In theſe alſo care has been taken that no woman is reckoned more than once, although many of them had been aſſiſted by the midwives to the Diſpenſary two, three, or four times. 370, as noted in the table, were in their firſt pregnancy.

† Of theſe 5419 children 2747 were boys, and 2672 girls, or nearly as 36 boys to 35 girls. This proportion of the boys to the girls will be found a little different from what is given in the table p. 362.

I have

I have placed theſe two tables together, that we might have an opportunity of obſerving how exceedingly fertile the women of the poorer claſſes in this country are; and at the ſame time how unable to rear any conſiderable number of children; for, although 321 of the women had borne ſix children and upwards each, and were all again pregnant, 19 only of them had been able to rear ſix or more children; and, although 102 of the women had borne nine children and upwards each, only one of them had been able to preſerve that number living.

I am inclined to believe, that this great mortality amongſt the children does not ariſe from any natural imbecillity or a conſtitution vitiated from the birth, many of thoſe victims being born with all the appearances of health and vigour; but that we ought rather to ſearch for the cauſe of it in the poverty of the parents, which prevents their taking the neceſſary care of, or even affording ſufficient cloathing and nouriſhment to their offspring. Whether this great check to population is in its nature irremediable; and whether an abatement in the pariſh rates and taxes, but particularly the former, to perſons rearing more than a certain number of children, or any other mode of relief and encouragement, would contribute to reſtrain ſo melancholy an evil, are inquiries well deſerving the attention of government. In order to determine how well my conjectures on this ſubject are founded, it might be uſeful, perhaps, to learn what the proportion of deaths is in more opulent families, where the cauſe juſt now mentioned can have but little influence. But this muſt be the reſult of the united obſervations of different practitioners.

I ſhall now from theſe tables attempt to collect what the chance of life is at different periods, from infancy to twenty-ſix years of age; but, that I may be underſtood, it will be

neceſſary to premiſe ſome account of the method I have followed.

I have ſuppoſed each of the women to bear a child every two years; this, from the account of thoſe who returned to the Diſpenſary a ſecond, third, or fourth time, appearing to be the mean term. Upon this principle, when I find that a woman applied at the Diſpenſary who had had one child before, I conclude, that that child would be two years old, if living; but if the woman had borne two children, I ſuppoſe that the firſt would be four, the ſecond two years old, and ſo on. And finding, that of 299 children borne by as many women, who were now advanced in their ſecond pregnancy, 171, or ſeven-twelfths only were living, I conclude, that on an average 5 out of 12 die under two years of age: and obſerving that of 508 children borne by 254 women, who were now advanced in their third pregnancy, 259 only were living, I firſt deduct 210, which is five-twelfths of the whole number, who died under two years of age; and then find that 39, which is nearly one-twelfth of the whole number, or one-ſeventh of the ſurvivors, died between two and four years of age.

TABLE

TABLE of the chance of life from infancy to 26 years of age.

Age.	Perſons living.	Decreaſe of life.	
0	5400	2250	5 in 12.
2	3150	450	6 in 12, or 1 in 7 of the ſurvivors.
4	2700	180	8 in 15, or 1 in 15 of the ſurvivors.
6	2520	204	4 in 7, or 1 in 12 and ½ of the ſurvivors.
8	2313	156	6 in 10, or 1 in 15 of the ſurvivors.
18	2160	540	7 in 10, or 1 in 4 of the ſurvivors.
26	1620		

3780 or ſeven-tenths would die.
1620 or three-tenths would be living at the end of 26 years.

5400

Whether this mode of calculating the chance of life will be admitted by gentlemen who have made ſpeculations of this kind their peculiar ſtudy, I know not. I confeſs, that when I firſt thought of it, I expected it would have proved more certain and accurate than upon examination I have found it to be: for, although in the firſt ſeries of years, where the deaths are numerous, the proportions agree tolerably well with the tables of M. BUFFON and others, yet as we advance we find ourſelves obliged to take longer periods than two years. Thus, for inſtance, we may obſerve, that although from 2 to 4, from 4 to 6, and from 6 to 8 years of age, the decreaſe continues to go on; yet ſo far is this from being the caſe between the ages of 8 and 10, or even 12, that there then appears to be ſome trifling increaſe. But as the proportion of deaths from 8 to 10 or 12 is probably inconſiderable, a very ſmall deduction from the deaths in the earlier years would rectify this difference. A deviation of this kind, I preſume, might be occaſioned by the ſmall-pox,

or

or ſome other epidemic, prevailing amongſt children during two or three years of the time I was making this collection, which would occaſion the decreaſe in the firſt and ſecond ſeries to be greater than uſual. If this ſhould prove to be the caſe, it is probable, that in a courſe of years, by comparing this with a variety of ſimilar tables, the true medium may be found.

A COMPARATIVE TABLE of the population of London, with a view to ſhew the proportion of natives to perſons born in the different counties of England and Wales, in Scotland, Ireland, or foreign countries.

Of 3236 married perſons

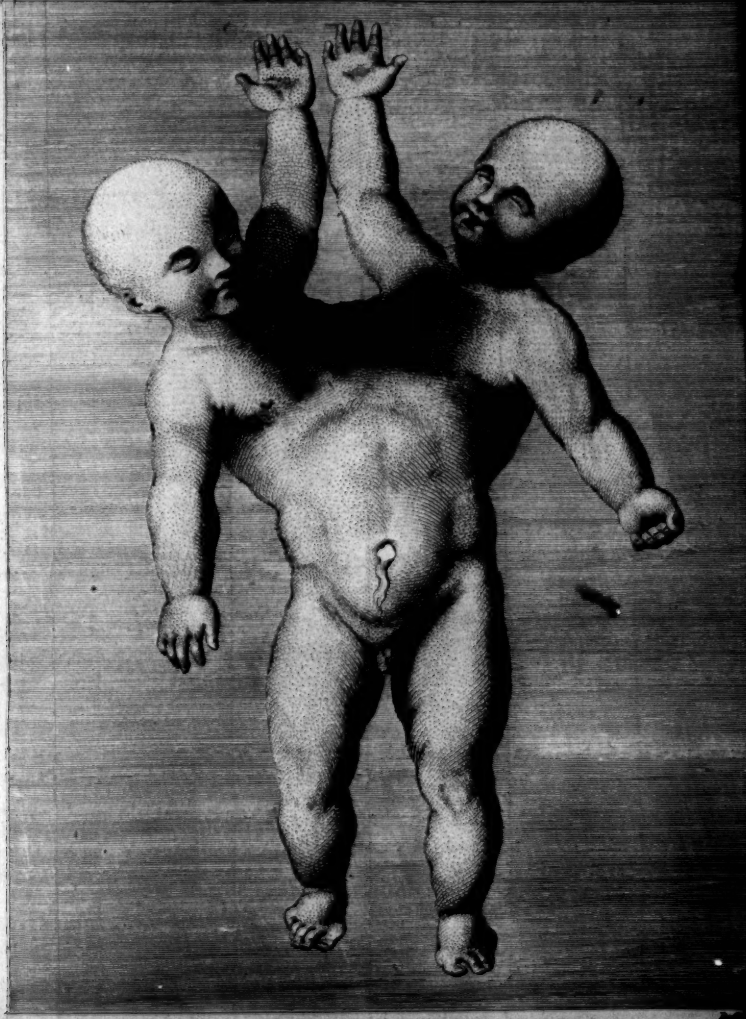
824 or one-fourth were born in London.
1870 or four-ſevenths in the different counties of England and Wales.
209 or 1 in 15 in Scotland.
280 or 1 in 11 in Ireland.
53 or 1 in 60 were foreigners.

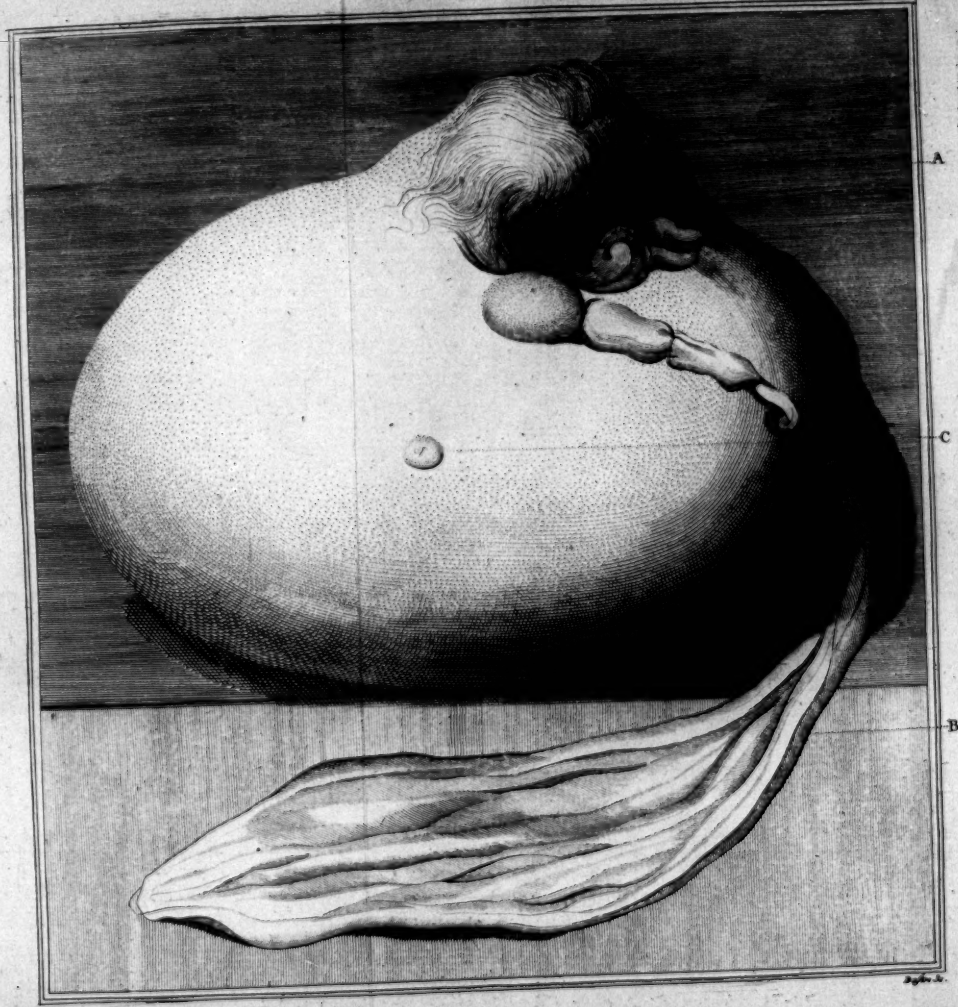
3236

Of the above number the males and females were in the following proportions.

Men.		Women.	
329	were born in London, and	495	or 166 more than men.
952	—— in different counties	917	or 35 fewer than men.
135	—— in Scotland,	74	or 61 fewer than men.
162	—— in Ireland, —	119	or 43 fewer than men.
40	—— were foreigners,	13	or 27 fewer than men.
1618		1618	166

Thus,




A
C
B

Thus, of 824 married perſons born in London, there were one-fifth more women than men. This may be accounted for either by ſuppoſing a greater number of males to die or to migrate before they attain a marriageable age than women. It is alſo to be obſerved, that of the Scotch and of the foreigners the women are in proportion to the men as about 1 to 3; but of the Iriſh they are as 3 to 7.

By this table we find at how great an expence to the country this city is maintained; and as we may ſuppoſe that the bulk of the Scotch, Iriſh, and foreigners, who come into the kingdom, reſide in the metropolis, we hence may alſo learn in what proportion they contribute to repair the waſte which is incurred by its exceſſive populouſneſs. A more compleat knowledge of theſe facts may give riſe to regulations which, if the calculations of Dr. PRICE ſhall be found to be juſt, are but too neceſſary; but I fear I have already intruded upon your patience, and extended this paper beyond its due bounds. I ſhall only add, that if theſe inquiries ſhould be favourably received by the illuſtrious body to whom you have ſo obligingly undertaken to preſent them, they will be continued, and their value of courſe increaſed by the additional number of objects which each year will ſupply.

I am, &c.

St. Alban's Street,
March 26, 1781.

XXIII. *Account of a Child who had the Small-pox in the Womb. In a Letter from* William Wright, *M. D. F. R. S. to* John Hunter, *Esq. F. R. S.*

Read May 21, 1781.

Southampton-buildings, Holborn,
Feb. 27, 1781.

SIR,

I HAVE read with much pleasure and information Mrs. FORD's case, which you published in Phil. Trans. vol. LXX. p. 128. From the facts you have adduced it amounts to a certainty, that her fœtus had received the variolous infection in the womb.

This induces me to lay before you a singular case, that fell under my care some years ago. I am sorry I cannot be more particular, having unfortunately lost all my books and my notes of practice of this case and several others, by the capture of the convoy on the 9th of last August.

In 1768 the small-pox was so general in Jamaica that very few people escaped the contagion. About the middle of June Mr. PETERKIN, merchant at Martha-brae, in the parish of Trelawney, got about fifty new negroes out of a ship: soon after they landed, several were taken ill of a fever, and the small-pox appeared; the others were immediately inoculated. Amongst the number of those who had the disease in the natural way, was a woman of about twenty-two years of age, and big with child. The eruptive fever was slight, and the small-pox

ſmall-pox had appeared before I ſaw her. They were few, diſtinct and large, and ſhe went through the diſeaſe with very little trouble, till on the fourteenth day from the eruption ſhe was attacked with the fever, which laſted only a few hours. She was, however, the ſame day taken in labour, and delivered of a female child with the ſmall-pox on her whole body, head, and extremities. They were diſtinct and very large, ſuch as they commonly appear on the eighth or ninth day in favourable caſes. The child was ſmall and weakly; ſhe could ſuck but little; a wet nurſe was procured, and every poſſible care taken of this infant, but ſhe died the third day after ſhe was born. The mother recovered, and is now the property of ALEXANDER PETERKIN, Eſq. in St. James's Pariſh.

In the courſe of many years practice in Jamaica, I have remarked, that where pregnant women had been ſeized with the natural ſmall-pox, or been by miſtake inoculated, that they generally miſcarried in the time of, or ſoon after, the eruptive fever; but I never ſaw any ſigns of ſmall-pox on any of their bodies, except on the child's above mentioned.

I am, &c.

XXIV. *Natural History of the Insect which produces the Gum Lacca. By Mr. James Kerr, of Patna; communicated by Sir Joseph Banks, P. R. S.*

Read May 24, 1781.

COCCUS LACCA.

Head and trunk. THE head and trunk form one uniform, oval, compressed, red body, of the shape and magnitude of a very small louse, consisting of twelve transverse rings. The back is carinate; the belly flat; the antennæ half the length of the body, filiform, truncated, and diverging, sending off two, often three, delicate, diverging hairs, longer than the antennæ. The mouth and eyes could not be seen with the naked eye.

Tail. The tail is a little white point, sending off two horizontal hairs as long as the body.

Feet. It has three pair of limbs, half the length of the insect.

I have often observed the birth of these insects, but never could see any with wings; nor could I find any distinction of sexes, nor observe their connubial rites: nature and analogy seem to point out a deficiency in my observations, possibly

owing to the minuteness of the object, and want of proper glasses.

Change. This insect is described in that state in which it sallies forth from the womb of the parent in the months of November and December. They traverse the branches of the trees upon which they were produced for some time, and then fix themselves upon the succulent extremities of the young branches. By the middle of January they are all fixed in their proper situations, they appear as plump as before, but shew no other marks of life. The limbs, antennæ, and setæ of the tail are no longer to be seen. Around their edges they are environed with a spissid subpellucid liquid, which seems to glue them to the branch: it is the gradual accumulation of this liquid, which forms a compleat cell for each insect, and is what is called Gum Lacca. About the middle of March the cells are completely formed, and the insect is in appearance an oval, smooth, red bag, without life, about the size of a small cuchanical insect, emarginated at the obtuse end, full of a beautiful red liquid. In October and November we find about twenty or thirty oval eggs, or rather young grubs, within the red fluid of the mother. When this fluid is all expended, the young insects pierce a hole through the back of their mother, and walk off one by one, leaving their exuviæ behind, which is that white membraneous substance found in the empty cells of the Stick Lac.

Place. The insects are the inhabitants of four trees.

1. Ficus Religiosa, LINNÆI. In Hindostan, Pipul. Banyan Tree.

2. Ficus Indica, LINNÆI. In Hindostan, Bhur. Banyan Tree.

3. Plaso Hortus Malabarici. By the natives, Praso.

4. Rhamnus Jujuba, LINNÆI. In Hindostanick, Beyr.

The insects generally fix themselves so close together, and in such numbers, that I imagine only one in six can have room to compleat her cell: the others die, and are eat up by various insects. The extreme branches appear as if they were covered with a red dust, and their sap is so much exhausted, that they wither and produce no fruit, the leaves drop off, or turn to a dirty black colour. These insects are transplanted by birds: if they perch upon these branches, they must carry off a number of the insects upon their feet to the next tree they rest upon. It is worth observing, that these fig trees when wounded drop a milky juice, which instantly coagulates into a viscid ropey substance, which, hardened in the open air, is similar to the cell of the Coccus Lacca. The natives boil this milk with oils into a bird-lime, which will catch peacocks or the largest birds.

A red medicinal gum is procured by incision from the Plaso Tree, so similar to the Gum Lacca that it may readily be taken for the same substance.

ſubſtance. Hence it is probable, that thoſe inſects have little trouble in animalizing the ſap of theſe trees in the formation of their cells. The Gum Lacca is rarely ſeen upon the Rhamnus Jujuba; and it is inferior to what is found upon the other trees. The Gum Lacca of this country is principally found upon the uncultivated mountains on both ſides the Ganges, where bountiful nature has produced it in ſuch abundance, that was the conſumption ten times greater the markets might be ſupplied by this minute inſect. The only trouble in procuring the Lac is in breaking down the branches, and carrying them to market. The preſent price in Dacca is about twelve ſhillings the hundred pounds weight, although it is brought from the diſtant country of Aſſam. The beſt Lac is of a deep red colour. If it is pale, and pierced at top, the value diminiſhes, becauſe the inſects have left their cells, and conſequently they can be of no uſe as a dye or colour, but probably they are better for varniſhes.

This inſect and its cell has gone under the various names of Gum Lacca, Lack, Loc Tree. In Bengal, La; and by the Engliſh it is diſtinguiſhed into four kinds.

1ſt. Stick Lac, which is the natural ſtate from which all the others are formed.

2. Seed Lac is the cells ſeparated from the ſticks.

3d. Lump

3d. Lump Lac is Seed Lac liquified by fire, and formed into cakes.

4th. Shell lac is the cells liquified, ſtrained, and formed into thin tranſparent laminæ in the following manner. Separate the cells from the branches, break them into ſmall pieces, throw them into a tub of water for one day, waſh off the red water and dry the cells, and with them fill a cylindrical tube of cotton cloth, two feet long, and one or two inches in diameter; tie both ends, turn the bag above a charcoal fire; as the Lac liquifies twiſt the bag, and when a ſufficient quantity has tranſuded the pores of the cloth, lay it upon a ſmooth junk of the Plantain tree (Muſa Paradiſiaca, LINNÆI), and with a ſtrip of the Plantain leaf draw it into a thin lamella; take it off while flexible, for in a minute it will be hard and brittle. The value of Shell Lac is according to its tranſparency.

Uſe to the natives. This is one of the moſt uſeful inſects yet diſcovered.

Ornaments for the ladies. The natives conſume a great quantity of Shell Lac in making ornamental rings, painted and gilded in various taſtes, to decorate the arms of the ladies; and it is formed into beads, ſpiral and linked chains for necklaces, and other female ornaments.

Sealing wax. Take a ſtick, and heat one end of it upon a charcoal fire; put upon it a few leaves of the Shell Lac ſoftened above the fire; keep alternately heating and adding more Shell Lac, until you

you have got a maſs of three or four pounds of liquified Shell Lac upon the end of your ſtick *. Knead this upon a wetted board with three ounces of levigated cinnabar, form it into cylindrical pieces; and, to give them a poliſh, rub them while hot with a cotton cloth.

Japanning. Take a lump of Shell Lac, prepared in the manner of ſealing-wax, with whatever colour you pleaſe, fix it upon the end of a ſtick, heat the poliſhed wood over a charcoal fire, and rub it over with the half-melted Lac, and poliſh, by rubbing it even with a piece of folded Plantain leaf held in the hand; heating the lacquer, and adding more Lac as occaſion requires. Their figures are formed by Lac, charged with various colours in the ſame manner.

Varniſh. In ornamenting their images and religious houſes, &c. they make uſe of very thin beat lead, which they cover with various varniſhes, made of Lac charged with colours. The preparation of them is kept a ſecret. The leaf of lead is laid upon a ſmooth iron heated by fire below, while they ſpread the varniſh upon it.

Grindſtones. Take of river ſand three parts, of Seed Lac waſhed one part, mix them over the fire in a pot, and form the maſs into the ſhape of a grindſtone, having a ſquare hole in the center, fix it on an axis with liquified Lac, heat the ſtone moderately, and by turning the axis it may eaſily be formed into an exact orbicular ſhape. Poliſh-

* In this manner Lump Lac is formed from Seed Lac.

ing

ing grindſtones are made only of ſuch ſand as will paſs eaſily through fine muſlin, in the proportion of two parts ſand to one of Lac. This ſand is found at Ragimaul. It is compoſed of ſmall angular cryſtalline particles, tinged red with iron, two parts to one of black magnetic ſand.

The ſtone-cutters, inſtead of ſand, uſe the powder of a very hard granite called Corune.

Theſe grindſtones cut very faſt. When they want to increaſe their power they throw ſand upon them, or let them occaſionally touch the edge of a vitrified brick. The ſame compoſition is formed upon ſticks, for cutting ſtones, ſhells, &c. by the hand.

Painting. Take one gallon of the red liquid from the firſt waſhing for Shell Lac, ſtrain it through a cloth, and let it boil for a ſhort time, then add half an ounce of ſoap earth (foſſil alkali); boil an hour more, and add three ounces of powdered load (bark of a tree); boil a ſhort time, let it ſtand all night, and ſtrain next day. Evaporate three quarts of milk, without cream, to two quarts, upon a ſlow fire, curdle it with ſour milk, and let it ſtand for a day or two; then mix it with the red liquid above mentioned; ſtrain them through a cloth, add to the mixture one ounce and an half of allum, and the juice of eight or ten lemons: mix the whole, and throw it into a cloth-bag ſtrainer. The blood of the inſect forms a coagulum with the caſeous part of the milk, and remains in the bag, while a limpid acid water

water drains from it. The coagulum is dried in the ſhade, and is uſed as a red colour in painting and colouring.

Dying. Take one gallon of the red liquid prepared as before without milk, to which add three ounces of allum. Boil three or four ounces of tamarinds in a gallon of water, and ſtrain the liquor. Mix equal parts of the red liquid and tamarind water over a briſk fire. In this mixture dip and wring the ſilk alternately until it has received a proper quantity of the dye. To increaſe the colour, increaſe the proportion of the red liquid, and let the ſilk boil a few minutes in the mixture. To make the ſilk hold the colour, they boil a handful of the bark called Load in water, ſtrain the decoction, and add cold water to it; dip the dried ſilk into this liquor ſeveral times, and then dry it. Cotton cloths are dyed in this manner; but the dye is not ſo laſting as in ſilk.

Spaniſh wool. The Lac colour is preſerved by the natives upon flakes of cotton dipped repeatedly into a ſtrong ſolution of the Lac Inſect in water, and then dried.

Uſe to the Europeans. See European authors.

Explanation of the figures.

a The Coccus Lacca at its birth, } natural size.
b Ditto, big with young, }

γ The embrio before birth inclosed in its membrane, }
δ The Coccus, with two hairs from each antenna, } magnified.
ε Ditto, with three hairs from each antenna, }

XXV. *Account of a Phenomenon observed upon the Iſland of Sumatra.* By William Marſden, *Eſq.*; *communicated by Sir* Joſeph Banks, *P. R. S.*

Read May 24, 1781.

SIR, Caroline-ſtreet, Feb. 24, 1781.

DURING my reſidence on the iſland of Sumatra in the Eaſt Indies, I had occaſion to obſerve a phenomenon ſingular, I believe, in its kind, an account of which may not perhaps be unintereſting to the curious.

In the year 1775 the S.E. or dry monſoon, ſet in about the middle of June, and continued with very little intermiſſion till the month of March in the following year. So long and ſevere a drought had not been experienced then in the memory the oldeſt man. The verdure of the ground was burnt up, the trees were ſtripped of their leaves, the ſprings of water failed, and the earth every where gaped in fiſſures. For ſome time a copious dew falling in the night ſupplied the deficiency of rain; but this did not laſt long: yet a thick fog, which rendered the neighbouring hills inviſible for months together, and nearly obſcured the ſun, never ceaſed to hang over the land, and add a gloom to the proſpect already but too melancholy. The Europeans on the coaſt ſuffered extremely by ſickneſs; about a fourth part of the whole number being carried off by fevers

and other bilious diſtempers, the depreſſion of ſpirits which they laboured under not a little contributing to haſten the fatal effects. The natives alſo died in great numbers.

In the month of November 1775, the dry ſeaſon having then exceeded its uſual period, and the S.E. winds continuing with unremitting violence, the ſea was obſerved to be covered, to the diſtance of a mile, and in ſome places a league from ſhore, with *fiſh* floating on the ſurface. Great quantities of them were at the ſame time driven on the beach or left there by the tide, ſome quite alive, others dying, but the greateſt part quite dead. The fiſh thus found were not of one but various ſpecies, both large and ſmall, flat and round, the Cat-fiſh and Mullet being generally the moſt prevalent. The numbers were prodigious, and overſpread the ſhore to the extent of ſome degrees; of this I had ocular proof or certain information, and probably they extended a conſiderable way farther than I had opportunity of making enquiry. Their firſt appearance was ſudden; but though the numbers diminiſhed, they continued to be thrown up, in ſome parts of the coaſt, for at leaſt a month, furniſhing the inhabitants with food, which, though attended with no immediate ill conſequence, probably contributed to the unhealthineſs ſo ſeverely felt. No alteration in the weather had been remarked for many days previous to their appearance. The thermometer ſtood as uſual at the time of year at about 85°.

Various were the conjectures formed as to the cauſe of this extraordinary phenomenon, and almoſt as various and contradictory were the conſequences deduced by the natives from an omen ſo portentous; ſome inferring the continuance, and others, with equal plauſibility, a relief from the drought. With reſpect to the cauſe, I muſt confeſs myſelf much at a

loſs to account for it ſatisfactorily. If I might hazard a conjecture, and it is not offered as any thing more, I would ſuppoſe, that the ſea requires the mixture of a due proportion of freſh water to temper its ſaline quality, and enable certain ſpecies of fiſh to ſubſiſt in it. Of this ſalubrious correction it was deprived for an unuſual ſpace of time, not only by the want of rain, but by the ceaſing of many rivers to flow into it, whoſe ſources were dried up. I rode acroſs the mouths of ſeveral perfectly dry, which I had often before paſſed in boats. The fiſh no longer experiencing this refreſhment, neceſſary as it would ſeem to their exiſtence, ſickened and periſhed as in a corrupted element.

If any thing ſimilar to what I have above deſcribed has been noticed in other parts of the world, I ſhould be happy by a compariſon of the attendant circumſtances, to inveſtigate and aſcertain the true cauſes of ſo extraordinary an effect. In communicating to you the obſervations I have made, I purſue the moſt likely means of obtaining this ſatisfaction.

I have the honour to be, &c.

XXVI. *Farther Experiments on Cold, made at the* Macfarlane *Obſervatory belonging to* Glaſgow College. *In a Letter from* Patrick Wilſon, *M. A. to the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read May 24, 1781.

DEAR SIR,

Glaſgow College, Feb. 9, 1781.

SOME days of very cold weather, which we had lately in this country, having afforded an opportunity of proſecuting a little farther the experiments and obſervations begun in the courſe of laſt year, I now do myſelf the pleaſure of communicating to you the following particulars, which perhaps may be conſidered as not unworthy of notice.

The froſt ſet in on Sunday the 21ſt of January, after a conſiderable fall of ſnow on the preceding evening, and about midnight the thermometers were expoſed near to the Obſervatory in the ſituations mentioned in my former letter. The following regiſter ſhews the difference of temperature between the ſnow and the air, till eight o'clock on Monday morning, to which are ſubjoined ſome facts which prove very conſonant to thoſe deſcribed in the former paper.

The ſign − prefixed denotes degrees below 0. The ſign + degrees above 0 of FAHRENHEIT's thermometer.

Monday

Monday morning, January 22, 1781.

h.	m.	Thermometer in air.	Thermometer on ſnow.
1		0	−12
1	30	+2	−12
1	45	0	− 8
2		−2	− 7
3		0	− 4
3	45	0	− 8
4		−1	−12
4	30	−3	− 8
5		−2	−12
6	15	−3	−13
6	45	−2	−10
7		−3	−13
7	30	−2	−10
8		−4	−11
8	30	−2	−10

From one o'clock till three in the morning the thermometer in air at the baluſtrade of the eaſt wing of the Obſervatory pointed from +4 to +6, and on the ſnow there from −2 to 0. At half an hour after one the thermometer in air, twenty-four feet from the ground, and to the windward of the houſe, pointed to +7, and at eight o'clock to +1. At three o'clock the ſnow in the park, three inches below the ſurface, raiſed the thermometer to +14, and at ſix inches below, near the ground, to +24. The barometer ſtood at 29.8 inches, and there was a perceptible motion of the air from the eaſt and one point ſouth. This night was a very general and lively aurora borealis, moſt

part of it of a bright red, which formed a crown near to the zenith; but it mostly vanished about three o'clock, after which time the air became more still. During the whole of this night, as well as of the succeeding times of observing, the air was not nearly so much disposed to give out hoar-frost as it was last year.

On Monday evening the difference of temperature was found to be as in the following register.

Monday evening,

h.	m.					Therm. in air.	Therm. in snow.
8		—	—	—	—	+16	+7
8	30	—	—	—	—	+14	+3
9		—	—	—	—	+ 8	+1
9	30	—	—	—	—	+ 7	+1
10		—	—	—	—	+ 7	+3
10	30	—	—	—	—	+ 6	+0
11		Ball of therm. ½ an inch above the surface of the snow,				+ 5	+3
12		Ditto,	—	—	—	+ 5	+3

Tuesday morning.

1		Ball of therm. as formerly half immersed in the snow,				+ 6	+3
2		—	—	—	—	+ 8	+5
2	30	—	—	—	—	+10	+6

No aurora this evening; the air very still and serene till about two o'clock Tuesday morning, when the wind rose remarkably, and clouds formed in the north-east.

On

On Thurſday, January 25, the difference of temperature was found to be as here ſet down.

Thurſday morning.

h. m.	Thermometer in air.	Thermometer in ſnow.
9 45	+ 10	+ 3
10	+ 10	+ 3
10 30	+ 14	+ 4
11	+ 14	+ 8
11 30	+ 17	+ 9
12	+ 20	+ 12
12 30	+ 22	+ 20
1 afternoon.	+ 25	+ 26
1 30	+ 27	+ 27

From ten till eleven o'clock this forenoon the thermometer on the baluſtrade in air, ſix inches above the ſnow, pointed to + 14, and when tried upon the ſnow to + 10. About noon this day ſome clouds were formed, which became quite general by one o'clock.

During the two laſt times of obſerving, three experiments were made with a view of diſcovering whether the ſnow without doors was gaining any thing from the air; or if any of it was carried off in the way of evaporation? For this purpoſe, a ſhallow diſh, made of ſheet braſs, four inches in diameter, was exactly filled with ſnow, and carefully weighed. In order to defend the outſide of the diſh from the air, that no hoar-froſt might attach itſelf to the metal, a circular hole was cut in the lid of a paſte-board box, ſo wide as juſt to let in the diſh

to the very brim, ſo that nothing communicated with the external air but the ſnow itſelf. The apparatus, in this ſtate, was ſet without doors for three hours each time, and then brought in to the lobby of the Obſervatory, where the diſh was again weighed: but in none of theſe trials did it ever appear, that any weight was loſt. On the contrary, at the firſt weighing, which was on Monday night, twelve o'clock, it had gained five grains. In the other two trials the increaſe of weight was ſcarce perceivable.

The temperature of the air in the weſt room of the Obſervatory remaining very conſtantly for near two days at $+27°$, a diſh of ſnow, ſimilar to the other expoſed there, was found to loſe weight very ſenſibly, and for the moſt part at the rate of two grains in an hour. Notwithſtanding this, the ſnow thus waſting or evaporating had no power of ſinking the thermometer below $+27$, the temperature of the ſurrounding air; though at one time it was fanned for four minutes by a piece of paper faſtened to the end of a long ſtick. Not to diſturb the uniform temperature of this room during theſe experiments, care was taken to ſtay in it a very ſhort time at every viſit, and to keep the door and the window-ſhutters cloſe.

On Chriſtmas-day we had a froſt, which in the morning made the thermometer in air point to $+21$; and during the preceding night there had been a profuſe depoſition of hoar-froſt. A pound of this was collected, and its capacity for heat compared to that of ice, and found equal as nearly as could be judged. Before making the two mixtures neceſſary for this experiment, the ice was reduced to a powder, and ſpread out on a paper beſide the hoar-froſt till both had acquired the ſame temperature.

On

On Monday night, January 22, about twelve o'clock, having occaſion to take up a little ſnow, there was obſerved a coheſion among its parts rather greater than what might have been expected in a ſubſtance, at that time, ſo much frozen. This circumſtance was farther examined by the following experiment. A pane of glaſs was laid on the ſurface of the ſnow till it had acquired the temperature of +3, after which, with a bit of parchment equally cold, ſome ſnow was ſcraped from the very ſurface, and ſhaken all over the pane, ſo as to cover it in moſt parts lightly. Upon now lifting the pane, and holding it with the ſnow undermoſt, the whole of it adhered, and it required ſome ſmart raps before the greater part fell away. What remained cleaved to the glaſs with ſtill a greater adheſion.

The experiments related above afford further reaſons againſt the opinion of the difference of temperature betwixt the ſnow or hoar-froſt and the air depending upon evaporation. It would moreover appear, that this phenomenon depends not either upon the depoſition of hoar-froſt. What renders this the more probable is, that on laſt year there was a much more copious depoſition at times when the difference of temperature was not more remarkable. But allowing that a depoſition had been found a neceſſary circumſtance, and always in proportion to that difference, the experiments on the capacities of hoar-froſt and ice ſeem to ſhew, that the ſenſible heat which diſappears enters not into the compoſition of the hoar-froſt; otherwiſe the capacity of this ſubſtance for heat, compared to that of ice or common ſnow, ſhould be very different. It muſt be confeſſed, however, that the above mentioned experiment would have been more applicable to this reaſoning, had it been made with hoar-froſt given out in colder ſtates of the air.

If the air, at low temperatures, had any power of acting upon the ſnow or hoar-froſt, ſo as to produce a gradual melting, this circumſtance, according to the known laws of heat, might occaſion the difference of temperature under conſideration. And what renders this idea not altogether improbable, is the peculiar coheſion among the parts of the ſnow above deſcribed. Perhaps a gentle melting might take place without much altering the appearance of the ſnow or hoar-froſt at the ſurface, as the parts, when diſſolved, might be gradually ſucked downwards, and be afterwards diſtributed through the whole drier maſs. It may alſo be worthy of an experimental inquiry to determine, how far that ſort of concretion, obſervable all over the ſurface of ſnow which has been long frozen, bears any marks of a ſlow proceſs of this kind. From a hill, a little way to the N.E. of the town, and which was to windward during the froſt, there were gathered two portions of ſnow, the one from the ſurface, and the other ſeven inches below it. The water produced from the two kinds is preſerved in very clean phials, in order to be compared together by ſome chemical trials, which, perhaps, may throw ſome light upon the whole of this matter.

At preſent I ſhall conclude this letter, perhaps already much too long, with juſt mentioning one other fact which was new to me; namely, the power of ardent ſpirits of diſſolving ſnow, and conſequently of producing with it a freezing mixture. The alcohol and ſnow ſeparately were at eight degrees below the freezing point, and when mixed ſuddenly and intimately, the temperature became in the ſpace of twenty ſeconds 28° below 0. This is a cold only 12° ſhort of that which FAHRENHEIT firſt produced by uſing ſpirit of nitre for the experiment; and

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it is not improbable, had the preſent experiment been tried with more precaution and addreſs, that the reſult would have been ſtill more remarkable. There was employed only about a pint of alcohol, but the proportion of ſnow was not then attended to, and the thaw coming ſoon afterwards prevented a repetition of the experiment.

I am, &c.

POSTSCRIPT.

I beg leave to add, that the water mentioned as produced from the ſuperficial ſnow has been examined by ſeveral chymical trials, with a view of diſcovering if it differed in any reſpect from the water obtained from ſnow gathered at conſiderable depths, and near the ground. Had the atmoſphere, when the thermometers pointed ſo low, been diſpoſed to furniſh any ſaline principle, the union of ſuch an ingredient with the ſnow would have tended to produce an exceſs of cold at the ſurface, ſimilar to what was then obſerved. Or if the ſnow at theſe low temperatures had acquired any remarkable power of dephlogiſticating the air in contact with it, a cooling proceſs at and near the confines of the ſnow and air might thereby have been maintained. In either of theſe caſes, ſome very ſenſible indications of a ſaline or of a phlogiſtic principle might be expected on the water given by the

the ſnow collected from the ſurface. But in oppoſition to both theſe views it remains now to be mentioned, that nothing of this kind did appear in the courſe of the experiments, which indeed were contrived chiefly to detect ſuch circumſtances.

If therefore the arguments produced in both papers upon this ſubject will not allow us to account for ſo remarkable a cooling proceſs by an evaporation at the ſurface of the ſnow, it would appear, that there remains ſtill ſomething unknown with reſpect to the cauſe. A proper inveſtigation of this matter, in climates favourable to ſuch experiments, may poſſibly unfold ſome farther properties of heat with which at preſent we may be wholly unacquainted.

XXVII. *A general Theory for the Menſuration of the Angle ſubtended by Two Objects, of which One is obſerved by Rays after Two Reflections from plane Surfaces, and the other by Rays coming directly to the Spectator's Eye.* By George Atwood, *M. A. F. R. S.*

Read June 21, 1781.

THE actual determination of an angle implies two obſervations, one taken at each extremity of the arc by which that angle is meaſured. When fixed aſtronomical quadrants or other ſectors are uſed for the practical eſtimation of angles, one of theſe obſervations is previouſly made by directing the axis of the teleſcope or line of collimation to ſome fixed point in the heavens, the index being then coincident with the initial point on the arc of the ſector: after this adjuſtment one obſervation only is neceſſary to aſcertain the angular diſtance

diſtance between that point and any other celeſtial object in the plane of the ſector. This method, however, is evidently impracticable, unleſs the inſtrument can be ſteadily fixed; for which reaſon aſtronomical quadrants become uſeleſs at ſea; and from the difficulties which attend placing them in their due poſition and adjuſtment on firm ground, they are almoſt wholly confined to regular obſervatories.

Mr. HADLEY *, by an ingenious application of optical principles, contrived to bring both extremities of the arc meaſured into the field of the ſpectators's view at the ſame time; by which improvement, angles are taken at ſea, as well as on land with an unfixed inſtrument, to a degree of accuracy ſufficient for nautical and other purpoſes, when the utmoſt exactneſs is not required.

Mr. HADLEY's invention is a particular caſe of a very extenſive theory, as yet but little attended to. According to his method, which is well known, the two reflecting ſurfaces uſed in the obſervation are perpendicular to the plane of motion; the direction of the teleſcope, and of the rays paſſing between the reflectors being parallel to that plane; whereas the inclination of the teleſcope, and of the intermediate rays, as well as of the reflectors themſelves to the plane of motion, admits of unlimited variety. A general theory to determine the angle obſerved by two reflections from the data on which its magnitude depends, without limitation or reſtriction, ſeems applicable to ſeveral uſeful purpoſes in practical aſtronomy. Having never ſeen any geometrical conſtruction or analyſis of this curious problem, I was induced to beſtow ſome conſideration

* Phil. Tranſ. N° 420. See alſo a tract, intituled, The Theory of HADLEY's quadrant, by the rev. W. LUDLAM.

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on the ſubject, and ſhall be happy if the reſult of my inquiries appears to merit the attention of the Royal Society.

Art. 1. The manner of taking an obſervation by two reflections unconfined to any particular caſe may be deſcribed thus. Let C, B (fig. 1.) repreſent two plane reflecting ſurfaces, inclined to a plane OPA at any given angle. Through any point of the reflecting ſurface C draw a line perpendicular to the plane OPA, and with the point where the line meets the plane as a centre (which muſt here be repreſented by C) and any diſtance CP, deſcribe a circle OPA. The reflecting plane B always continuing fixed, let the reflector C be moveable along with the radius CP as it revolves in the plane OPA round the centre C: the angular motion of the ſpeculum C, referred to the circumference OPA, will be meaſured by the arc which the radius CP deſcribes, the inclination of the plane C to the plane of motion OPA being always the ſame, and equal to that of the fixed ſpeculum B.

2. The two plane reflectors, B and C, being equally inclined to the plane OPA, it follows, that during the motion of C there muſt be ſome point O in the circumference OAP, at which when CP arrives, the reflector C will be parallel to the fixed reflector B.

3. When the moveable radius which carries round the plane C is at any other poſition CP, let a ray flowing from a diſtant object T impinge on the ſpeculum C; let it be reflected from thence in the direction CB, and being again reflected at B in the direction BG, let it be obſerved by a ſpectator's eye at G; the image of T will appear ſomewhere in the line GBS; ſuppoſe that a ray flows from a diſtant object S ſituated in the line GB produced, and that this ray SG comes directly to the ſpectator's

eye at G: the object S ſeen by direct rays, and the image of the point T ſeen by rays after two reflections, will appear to coincide in the line GBS. This is an obſervation by two reflections, from which, together with ſuch data as limit the problem, the true angle ſubtended by the objects T and S is to be inferred.

4. The data which limit this problem, being neceſſary for the determination of the angle ſubtended by T and S are in number four, which are next to be conſidered. 1ſt. One of theſe data is the arc PO, being the angular diſtance of the moveable radius CP, meaſured on the circumference of the circle OPA, from that poſition CO, at which the two reflectors are parallel; the ſituation of this arc OP in reſpect of the point O being ſuppoſed known, that is, it being known on which ſide of that point, OP is ſituated in reſpect of the ray BG. 2dly, The common inclination of the reflecting planes B and C to the plane of motion is another of theſe data. The third and fourth of the conditions muſt be mentioned rather more particularly. The ray BG is always underſtood to be given in poſition in reſpect of the plane of motion OPA (conſidered as immoveable) being either coincident with the line of collimation of a teleſcope, or directed by ſights ſo as to be invariably fixed: the ſpeculum B alſo being unmoved, the line or ray BC will never change its poſition, from the known principles of reflection. The angle CBG, therefore, and the half of that angle being the angle of incidence at which CB impinges on B, will be always of the ſame magnitude; whereas the half of the angle BCT, or the angle of incidence on the moveable ſpeculum C, is continually changing, while C is carried round in the plane of motion: this conſtant angle of incidence or reflection at the fixed ſpeculum B will be another of the data neceſſary to determine the problem.

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The rays GB, BC, and the ſpeculum B, being fixed in reſpect of each other, and of the plane OAP, the plane CBG will alſo be given in poſition; that is, its inclination to the plane of motion, or to any other fixed plane, will conſtantly be the ſame: whereas the inclination of the plane BCT to the plane of motion, or other fixed plane, will be continually changing while the reflector C revolves with the radius CP. The poſition of the plane GBC conſtitutes the fourth and laſt of the data, and it will be immaterial to what fixed plane it is referred. In the enſuing ſolution the ſituation of this plane will be defined by its inclination to the fixed ſecondary of the plane of motion which paſſes through the point O.

5. The enumeration of theſe data leads to the conſtruction of the problem, a few obſervations being previouſly inſerted to prevent repetitions and unneceſſary references. 1ſt, The objects obſerved are underſtood to be lucid or illumined points, and ſo diſtant, that the rays which flow from either of them may be eſteemed parallel without error as far regards theſe obſervations: ſuch objects are the fixed ſtars, any given points in the diſks of the ſun or planets, &c. 2dly, As in meaſuring the angular poſitions of objects which lie in the ſame plane, theſe objects are referred to the circumference of a circle, the centre of which is coincident with the ſpectator's eye; ſo in eſtimating the poſitions of objects which lie in different planes, and of the inclinations of theſe planes to each other, the objects, &c. are referred to the circumference of a ſphere, of which the centre coincides with the centre of the ſpectator's view: applying this to the preſent caſe, ſince the lines CT, CB, SG (fig. 1.) are ſituated in different planes; in order to eſtimate their poſitions, any point may be aſſumed as the centre of a ſphere, and through that point lines are to be drawn parallel to the given lines CT, CB, SG, the points in

which the lines interſect the ſpheres ſurface will give their relative ſituations by the rules of trigonometry. 3dly, There will be no neceſſity to repreſent the reflecting planes in the general conſtruction, ſince the poſitions of the perpendiculars to the planes will give the ſituations of the planes themſelves.

6. To determine by conſtruction the angle ſubtended by the objects T, S, from the data which have been deſcribed, let APOCQ (fig. 2.) repreſent a great circle of the ſphere to the ſurface of which the objects obſerved, and the poſitions of the incident and reflected rays, &c. are referred; C being the center, CK the axis, and K the pole of this great circle; through K draw any ſecondary KO, and from the pole K, at the diſtance of the arc KF, = the meaſure of the given inclination of the reflecting planes to the plane of motion, deſcribe a parallel or leſſer circle FIM: with the pole F, and at a diſtance equal to a quadrant, deſcribe an arc of a great circle interſecting the ſecondary KO produced in the point X, and in this arc from X take XY = the meaſure of the given inclination of the fixed plane of reflection at the ſpeculum B to the ſecondary which paſſes through the point O; and draw the quadrant YF, which produce in the direction YF: from F on either ſide of F ſet off FD equal to the meaſure of the given conſtant angle of incidence at the ſpeculum B, and make FB (taken on that ſide of F which is oppoſite to D) equal to FD. Draw the radius CO: from O ſet off an arc OP in the circumference OPA equal to the meaſure of the angular diſtance deſcribed by the moveable radius CP from that poſition at which the reflectors are parallel; obſerving that the arc OP be on that ſide of the point O which * correſponds with the conditions of the problem (art. 4.): through P deſcribe the ſecondary KP interſecting the parallel FIM in the point I: through B and I deſcribe

* It is ſuppoſed to be known, whether CP beginning its motion from the poſition CO approaches towards the viſual ray BG or recedes from it.

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the arc of a great circle BIE, and in it take EI equal to IB: through D and E draw the arc of a great circle DE: the arc DE will be the meaſure of the true angle ſubtended by the objects obſerved, according to the data of the problem.

Previous to the demonſtration of this conſtruction, the application of it to the method of obſervation by two reflections ſhould be deſcribed. Join CP, CI, and CF. To the extremity C of the radius CP let a plane ſpeculum be affixed, CI being always perpendicular to this plane: as PC revolves in the plane of motion, the perpendicular CI will deſcribe the parallel or leſſer circle FIM, and when CP coincides with CO, CI will coincide with CF. Through B draw BR parallel to CF, and let a plane ſpeculum be fixed at B perpendicular to BR; CF and BR being parallel when the perpendicular CI coincides with CF, the reflectors at C and B will then be parallel.

Join CD, and produce it to a very diſtant point S, and through B draw GBS parallel to CDS; the reflectors C and B being parallel, and their perpendiculars coinciding with CF and BR, let a ray SC impinge on the reflector C: becauſe FC is the perpendicular to the ſpeculum C and the arc DF = FB by conſtruction, theſe arcs being in the plane of the ſame great circle DBQ, it follows, that the ray SC will be reflected from C in the direction CB, impinging on the ſpeculum B at the angle of incidence CBR; and ſince DC and BG are parallel by conſtruction, and the parallel lines FC BR fall on them, the angles RBG, FCD, will be equal, and FCB or CBR = RBG. CB therefore being the ray incident on the ſpeculum B will be reflected in the direction BG parallel to SC; and a ray SG coming directly from S will be ſeen coincident with the reflected ray BG. Here we obſerve, that the planes of reflection at C and B, that is, the planes DCB and CBG coincide, the reflectors being parallel.

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Let the radius CP move from the poſition CO, carrying with it the ſpeculum C and its perpendicular CI: then, EI being equal to IB by conſtruction, a ray impinging on C in the direction TEC will be reflected in the plane ECB, and becauſe ECI = ICB, the reflected ray will coincide with the line CB, and after reflection at B will * proceed in the direction BG, being coincident with the ray SG which comes directly from S. When the perpendicular CI leaves CF, the plane of reflection ICB becomes inclined to the plane of reflection DCBG with which it before coincided; but the poſition of the rays CB, BG, and of the perpendicular BR, remains unaltered; for which reaſon the plane GBCFD correſponds to the fixed plane of reflection deſcribed among the conditions (art. 4.). When CI was coincident with CF, the radius CP was coincident with CO, O being the initial point of the arc OP, deſcribed by the radius CP, denoting that when CP coincides with O, the reflectors being then parallel, the inclination of the ray SC obſerved after two reflections, and SG obſerved by direct rays parallel to SC, is nothing: the great circle KO, therefore, which paſſes through O and F, will be the fixed or primitive ſecondary to which the inclination of the fixed plane of reflection at the ſpeculum B is referred.

The demonſtration of the conſtruction will conſiſt of two parts. It muſt be firſt ſhewn, that the conditions or data of the problem are obſerved in the conſtruction. 2dly, That the magnitude of the arc ED, which meaſures the angle ſubtended by the obſerved objects is limited or determined by them.

Suppoſing the angle TCS to be of any unknown quantity, it has appeared, that according to the conſtruction, the rays which come from T, and are ſeen after two reflections at C and B, will be obſerved to coincide with the rays which come

* Supra.

directly from 8. That the conditions of the problem are fulfilled in the conſtruction is demonſtrated thus;

1ſt. The inclination of the reflectors B and C to the plane of motion was conſtructed of the magnitude which is meaſured by the arc KF. KC is perpendicular to the plane of motion. CF is perpendicular to the reflector C, and the inclination of theſe two lines CK, CF, is meaſured by the arc KF; but the inclination of any two planes is the ſame as the inclination of two lines which are perpendicular to them; the inclination therefore of the reflector C to the plane of motion is meaſured by the arc KF, and the ſpeculum B is equally inclined to the plane of motion with C by the conſtruction, the perpendiculars CF and CI being parallel when both are ſituated in the plane of the ſame great circle DBQ.

2dly, KO being the ſecondary to which the poſition of the fixed plane of reflection DFB at the ſpeculum B was referred, that given inclination will be equal to the angle OFB, which is meaſured by the arc XY according to the conſtruction, FY being a quadrant.

3dly, Moreover, FD = FB, was conſtructed equal to the conſtant angle of incidence at the fixed ſpeculum; CBR is the angle of incidence at the fixed ſpeculum B, and it is equal to the angle BCF, becauſe CF and BR are parallel by conſtruction, and CB falls on them; FB, or its equal FD therefore is truly conſtructed the meaſure of the given conſtant angle of incidence at the fixed ſpeculum B.

4thly, Becauſe it has been ſhewn that CO is the poſition of the radius CP, when the reflectors are parallel, the arc OP is rightly conſtructed the meaſure of the angular diſtance of the radius CP from that poſition.

It remains only to demonſtrate that theſe four given quantities, KF, OP, XY, and DE, limit the magnitude of the arc

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ED: through I and F draw the arc IF: then the given arc KF or KI, and the angle IKF, meaſured by the given arc PO, define the triangle IKF, and in it, therefore, the ſide IF and and the angle IFK are determined. If from IFK, the given angle DFK, meaſured by the arc XY, be ſubtracted, the remainder IFD, and IFB its ſupplement to 180° will be defined: the given arc FB, with the angle IFB and the arc IF, determine the angle IBF, and the arc IB, or its double BE: and the given arc BD, the arc BE, with the contained angle DBE before determined, define the arc ED, which is therefore the true meaſure of the angle ſubtended by the objects obſerved under the conditions fulfilled in the conſtruction.

7. The computation of the obſerved angle DCE being for the preſent omitted, ſome conſequences which follow from the conſtruction may be inſerted in this place, being either corollaries, or ſuch truths as admit of eaſy geometrical deduction from the general propoſition. The line DC will always be the poſition of the viſual ray or line of collimation of the teleſcope uſed in the obſervation, and the inclination of it to the plane of motion will be meaſured by the complement of the arc DK to a quadrant. The line BC will be the poſition of the ray which paſſes between the reflectors B and C, and the inclination of it to the plane of motion will be meaſured by the complement of the arc BK to a quadrant. Theſe arcs are left out of the figure, that the more material parts of the conſtruction might not be confuſed by them.

8. Every thing elſe remaining, let the parallel FIM (fig. 3.) be projected on the plane of motion QOP. Through the points F and I draw the arc of a great circle NIFR. The obſerved * objects T and S, or, which is the ſame thing, the points of interſection at the ſphere's ſurface E and D will be at equal

* Compare fig. 2.

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perpendicular diſtances from this arc, which may be demonſtrated thus. Through the points E, D, and B, draw the arcs EN, DL, and BR, perpendicular to NIFR: then the triangles DFL, FBR, being equal, DL will be equal to BR; moreover, the triangles ENI, IRB, being equal, the arcs EN, RB, will be equal: from whence it follows, that EN = DL, or the perpendicular diſtances of the points E and D from the arc of a great circle which paſſes through the points I and F, are equal. It appears alſo, from the ſame conſtruction, that the arc NL, intercepted between the two perpendiculars EN, DL, is equal to twice IF: for becauſe the triangles EIN, RIB, are equal, as are the triangles DLF, RFB, it follows, that NI is equal to IR, and LF to FR, wherefore 2IR = NR, and 2RF = LR: whence, by ſubtracting equals from equals, 2RI − 2RF = NR − LR, or 2IF = NL, which was the equality to be demonſtrated.

9. From this laſt conſtruction and demonſtration the following proportion is inferred. As radius : coſine of DL or EN, ſo is the ſine of IF to the ſine of half the arc ED, or of half the obſerved angle: for if the arcs NE, LD (fig. 3.), be continued until they meet in the pole H, the arcs NH, LH, will be quadrants, and the triangle EHD iſoſceles, which, from a property of ſpherics too obvious to need demonſtrating, gives this proportion: as the chord of NL to the chord of ED, ſo is radius to the ſine of DH, or coſine of DL; but the chord of NL is equal to the chord of 2IF from art. 8. We have, therefore, as radius : coſine DL, ſo is the chord of 2FI to the chord of ED, or, which is the ſame proportion, as radius : coſine DL, ſo is the ſine of IF to the ſine of half ED.

10. From the laſt article it appears, that the ſine of half the angle between the obſerved objects, or the ſine of half ED,

is proportional to the ſine of FI and the coſine of DL jointly; conſequently the ſine of FI being the ſame, ſin. ½ ED is proportional to the coſine of DL; this will lead to the reaſon why in enumerating (art. 4.) the * conditions which limit the magnitude of the obſerved arc ED, the poſition of the ſecondary KP, in reſpect of the point of interſection Q and of the fixed ſecondary KO, was annexed: for it will appear, that every thing elſe being the ſame, the magnitude of the arc ED will depend on the poſition of the ſecondary KP, whether it be on one ſide of the fixed ſecondary KO, or on the other, the angles PKO, *p*KO, being equal. Having ſet off O*p* = OP draw the ſecondary K*p* interſecting the parallel FIMU in the point U; and through B and U draw the arc of a great circle BUW; take UW = BU; and through D and W draw the arc of a great circle DW: then by the conſtruction and demonſtration in art. 6. the angle ſubtended by the obſerved objects will be meaſured by the arc DW, and it will be eaſy to ſhew, that DW is not equal to DE, except in two extreme caſes; that is, when the fixed plane of reflection DFB is either coincident with the primitive ſecondary KO or perpendicular to it. Through the points F and U draw the arc of a great circle VFU, and from D draw the arc DV perpendicular to VFU: ſince † the ſines of half the arcs DE, DW, are in a proportion compounded of the proportions of the ſine of IF to the ſine of FU, and of the coſine of DL to the coſine of DV, the ſines of IF, FU, being equal by the conſtruction, the ſines of half the arcs ED, DW, will be in the ſame proportion with the coſines of DL and DV, which are evidently unequal; conſequently, the ſines of half the arcs DE, DW, and therefore the arcs themſelves, muſt be unequal.

11. The angles PKO, OK*p*, remaining equal, when the fixed plane of reflection BFD (fig. 4.) is coincident with the

* Compare fig. 2. † Supra.

ſecondary

ſecondary KO, or at right angles to it, the perpendiculars DL, DV, become equal in both caſes, which is obvious from the equality of the triangles DVF, DLF; it follows, therefore, (art. 10.) that the ſines of FI and FU, and the coſines of DL, DV, being equal, the arcs DE, DW, will be equal in theſe two extreme caſes, but in no other.

12. Since the angle ſubtended by the obſerved objects (art. 10.) depends only on the ſine of IF and the coſine of DL, it is plain, that if the points D and B be interchanged, (fig. 2, 3, 4.) the angle obſerved will not be altered, every thing elſe remaining the ſame; becauſe neither the ſine of IF, nor the coſine of DL, is affected by this change. For this reaſon in any conſtruction for meaſuring angles by two reflections, the poſition of the * viſual ray may be altered into that of the ray BC paſſing between the reflectors, which will become in that caſe the ſituation of the viſual ray, this alteration noways affecting the obſerved angles.

13. While the perpendicular CI (fig. 2. and 5.) deſcribes the parallel FIM, the angle of incidence on the moveable ſpeculum C, that is, the angle ECI or ICB, meaſured by the arc BI, continually increaſes until it arrives at a certain limit. This limit is determined by drawing through the points B and K the arc of a great circle BKM. When the perpendicular CI arrives at M, the arc BM is the greateſt poſſible, which will therefore be the meaſure of the greateſt angle of incidence on the moveable ſpeculum, according to this conſtruction, the radius CP having then deſcribed from O an arc which is the meaſure of the angle FKM. Now it is plain, that if the arc MB ſhould be greater than a quadrant, there can be no viſion by two reflections, when the perpendicular CI coincides with M (ſuppoſing the moveable ſpeculum to reflect on one ſide only) becauſe the angles of incidence and reflection on any ſpeculum

* The poſition of the ray DC is the ſame with that of the ray BG parallel to it, when referred to diſtant objects.

muſt be leſs than 90°. If BM be leſs than a quadrant, an obſervation by two reflections may be taken when the radius CP is directed to any point in the circumference of the plane of motion. When the arc BM is greater than a quadrant, two other limits will be produced in the circumference of the plane OCP; while the radius CP is between theſe limits, no obſervation by two reflections can be taken: theſe limits are conſtructed thus (fig. 6.). BM being greater than a quadrant, with the pole B and diſtance BI equal to a quadrant, deſcribe the arc of a great circle I*i* interſecting the parallel FIM in the points I and *i*: through I and *i* draw the ſecondaries KY, KZ: while the radius CP is between Z, and Y no obſervation can be taken by two reflections. If BIE, B*i*E, be drawn equal to a ſemi-circle, and DE joined, then DE will be the meaſure of the limiting angle which can be obſerved by this conſtruction, either on one ſide of KO or on the other; and becauſe, by the principles of trigonometry, the arcs BD and DE are in the ſame great circle, BDE being a ſemi-circle, we ſhall derive from the conſtruction this concluſion: the difference between 180° and double the angle of incidence on the fixed ſpeculum, will be a limit which terminates the angle obſerved by two reflections in every caſe, when the arc BM is greater than a quadrant*.

14. In any given example formed on the principles which have been demonſtrated (fig. 2.) for the eſtimation of angles by two reflections, three of the four quantities neceſſary to determine the reſult muſt conſtantly be the ſame, while the fourth, that is, the arc OP, varies with the magnitude of the angle ſubtended by the objects obſerved: the different magnitudes of theſe three given quantities will cauſe a great variety of properties in conſtructions which depend on the general

* This termination of the angle which can be obſerved by two reflections may happen while the obſerved angle is increaſing or decreaſing during the revolution of the index in the plane of motion.

theory. If the angle DFK (fig. 2.), being the inclination of the fixed plane of reflection to the primitive secondary be $= 90°$, and the arc KF, or the inclination of the reflectors to the plane of motion, be $= 90°$ alſo, the conſtruction will become that of HADLEY's inſtrument (fig. 7.), whatever be the magnitude of the arc DF, that is, of the angle of incidence on the fixed ſpeculum B: in this caſe the points F and O, and the points I and P, coincide. Here IF or PO meaſures the inclination of the reflectors to each other; and becauſe BF = FD, and BI = IE, by conſtruction, it follows, that DE = 2PO, that is, the angle ſubtended by the obſerved objects is double to the angle at which the reflectors are inclined to each other. This is a known property of HADLEY's inſtrument, in which the viſual ray, and the ray intermediate between the reflectors, are in the plane of motion, which is alſo expreſſed in the conſtruction, DC and BC coinciding with the plane POC.

15. Biſect KO in F; then will KF = 45° (fig. 8.). The viſual ray CD being coincident with the plane of motion, let the inclination of the reflectors to that plane be equal to 45°: moreover, let the angle * DFK = 180°; ſo ſhall D coincide with O, and B with K: this will afford a good example to the general theory. Let the radius CP move into any given poſition, carrying with it the ſpeculum C and its perpendicular CI: here the obſerved object E and the point B are always equi-diſtant from I; and becauſe BI is half a quadrant by conſtruction, it follows, that IE will be of the ſame magnitude, BE therefore will be a quadrant, and conſequently E will coincide with P, being always in the plane of motion. The following properties are alſo derived from this conſtruction. 1ſt, The arc DE ſubtended by the obſerved objects is equal to the arc deſcribed by the index or moveable radius CP from O;

* Compare fig. 2.

differing

differing in this from Mr. HADLEY's conſtruction, in which the angle obſerved is equal to double the angle deſcribed by the moveable radius from the initial point of the arc O. While therefore the moveable ſpeculum C is carried round by the radius CP in the plane of motion according to the new conſtruction juſt deſcribed, the image of E moves with an angular velocity juſt equal to that of the radius, the motion of the image being, according to Mr. HADLEY's invention, always greater than that of the radius in the proportion of 2 to 1. 2dly, The angles of incidence and reflection on both ſurfaces are conſtantly the ſame, being equal to 45°. 3dly, BI (art. 13.) being always loſs than 90°, obſervations by two reflections may be taken all round the circle, that is, angles of any magnitude may be meaſured by this conſtruction. It will not be difficult in practice to regulate the inclination of the plane reflectors to the plane of motion, with the other given quantities to their true magnitude. Let the reflectors B and C be brought parallel when the index or radius CP is directed to O, being the initial point of the arc OP: in order to examine whether the fixed plane of reflection BFD be coincident with the primitive ſecondary KO, it is only neceſſary to obſerve the angle ſubtended by two given objects when the index CP is on the different ſides of the initial point O: if the index be directed to unequal diſtances from that point at the times of obſervation, a correction is required (art. 11.). To examine whether the inclination of the reflectors to the plane of motion be exactly 45°, let the index CP be directed to 180°: if the inclination of the reflectors to each other be not then = 90°, a correction muſt be applied. It will be known whether the inclination of the reflectors to each other be = 90°, by obſerving the two oppoſite horizons at ſea, and at land by various obvious methods. Theſe examinations

are

are wholly independent of the inclination of the teleſcope to the plane of motion, which is regulated to its true ſituation parallel to the plane OPA, by making the plane OPA fixed in regard to diſtant objects, and by obſerving if the images of objects E, ſeen after two reflections of the rays, deſcribe the arc of a great circle while C is carried round the plane of motion. Any three fixed objects, at a ſufficient diſtance, and ſituated in the ſame plane with the obſerver's eye, will be ſufficient for making this adjuſtment.

Fig. 9. 10. and 11. repreſent the progreſs of the rays, and the poſition of the reflectors according to this conſtruction. TC is a ray iſſuing from any object T in a direction parallel to the plane of the motion, and impinging on the ſpeculum C, which is inclined to that plane at an angle of 45°: from hence it is reflected in the direction CB perpendicular to the plane OCA, and being there reflected by the ſpeculum B proceeds in the given or conſtant direction BG parallel to the plane of motion OPA.

16. There is another conſtruction which follows from the general theory, the deſcription of which ſhould not be omitted. This will require ſome little explanation. As before (fig. 12.) let OPA repreſent the plane of motion, K its pole, FIM a parallel or leſſer circle projected on it, the diſtance of this parallel from the pole K being meaſured by the arc FK; let KO be the primitive ſecondary, and BFD the fixed plane of reflection on the ſpeculum B coincident with it. The other parts of the conſtruction * remaining, it has been demonſtrated (art. 10.), that the ſine of half the obſerved angle, that is, the ſine of half ED, is proportional to the ſine of IF, and the coſine of DL jointly. Every thing elſe being the ſame, it is manifeſt, that the ſine of half ED will be proportional to the ſine of IF: as therefore the arc KF, that is, the inclination of the reflecting planes to the plane of motion, is decreaſed,

* Compare fig. 2. and 3.

the

the angle meaſured or arc ED will become ſmaller at the ſame time, becauſe FI decreaſes with KF, the angle IKF remaining. This property ſeems applicable to good purpoſe in meaſuring ſmall angles, not only from the great extent of ſcale, which is here obtained, but from various advantageous circumſtances, which will appear in the ſubſequent article, and from the computations annexed in thoſe which follow.

In this conſtruction the fixed plane of reflection is made coincident with the primitive ſecondary for various reaſons: there are only two poſitions of that fixed plane which admit of eaſy and exact adjuſtments; theſe are when the fixed plane of reflection is either perpendicular (art. 11.) to the primitive ſecondary or coincident with it. The latter poſition is preferred excluſive of the advantages it poſſeſſes in common with the other, becauſe it affords means for a very preciſe adjuſtment of the inclination of the reflecting planes to the plane of motion, that is, of the arc KF; for if the primitive ſecondary OKMD (fig. 12.) be produced, and in it DG be taken equal to four times KF, it is manifeſt, that when the perpendicular CI coincides with M, or, which is the ſame thing, when the radius CP is directed to 180°, the object E obſerved by two reflections will coincide with G, becauſe BF = FD and BM = MG by conſtruction. If then two given objects be obſerved when the index points to 180°, the inclination of the plane reflectors to the plane of motion will be one fourth part of the angle ſubtended by theſe objects.

Concerning the magnitude of the arc FB, being the meaſure of the angle of incidence on the fixed ſpeculum, and of KF = the inclination of the reflectors to the plane of motion, it will appear, by the computations*, that the ſmaller they are both taken, every thing elſe being the ſame, the more exact will be the reſult of the obſervation; but both are limited by circumſtances which

* Infra.

ſhould

ſhould next be deſcribed; theſe will be more obvious if an outline be annexed, repreſenting this particular caſe of the theory adapted to the menſuration of ſmall angles when reduced to practice. OAPC (fig. 13.) is the plane of motion, C the moveable ſpeculum carried round in the plane of motion by the radius CP; a ray coming from any diſtant object T impinges on the ſpeculum C, and being reflected in the direction CB, is there again reflected in the direction BG, paſſing along the axis of a teleſcope. A ray coming from another diſtant object S, inclined to the ray TC at a ſmall angle enters the teleſcope parallel to the direction of its axis, which is coincident with BG, and conſequently the images of the two objects S and T will be ſeen to coincide in the middle of the field of the teleſcope, the angle ſubtended by them being BGT, which muſt be determined by the ſubſequent computations.

17. By this figure (fig. 13.), without further argument, it is plain, that the magnitude of the reflecting ſpeculum C limits the conſtant angle of incidence on B; for were that angle $= o$, the lines CB, BG, would coincide, by which means the ray BG and others adjacent to it would be intercepted from entering the teleſcope. The magnitude of the reflecting plane depends on the quantity of light required; if a circle of about 1 2 inches diameter be ſufficient, and the perpendicular diſtance of the reflecting planes be made equal to five inches, the leaſt angle of incidence, conſiſtent with theſe conditions, will be about 7°. It is however to be remembered, that the magnitude of the reflectors ſhould be adapted to the aperture of the teleſcope uſed in the obſervation. As the area of the ſpeculum increaſes, the light admitted into the ſame teleſcope decreaſes, and theſe areas ſhould be ſo proportioned as to afford equal quantities of light, ſo that the objects ſeen by two reflections, and by direct rays, may be nearly of equal brightneſs; but for the

ſake of conſtructing an example to this theory, the magnitude of the reflecting planes and the angle of incidence on the fixed ſpeculum B depending on it may be aſſumed of the value mentioned in this article.

18. The magnitude of the arc KF, or of the inclination of the reflecting planes to the plane of motion is limited by the angles which the obſerved objects ſubtend (art. 16.). Fig. 12. Becauſe the greateſt angle obſervable will be meaſured by four times the arc KF, it follows, that the arc KF muſt not be leſs than one fourth part of the greateſt angle intended to be obſerved by this conſtruction; if the inclination denoted by the arc KF be fixed at 10′, four times that angle being 40′ will be greater than the apparent diameters of the ſun or any of the planets.

19. It remains to infer from the preceding conſtruction (fig. 2), the actual meaſure of the angle ſubtended by the objects obſerved. This muſt be effected by computation, which will not only ſerve as an illuſtration of the theory, but afford means of eſtimating and comparing the errors in the angle deduced, occaſioned by the unavoidable errors in obſervation and practical conſtruction; an examination extremely uſeful in aſtronomical ſubjects: next to removing errors entirely from obſervations, which is ſcarcely to be hoped for, the leſſening, circumſcribing, and reducing them within known limits is an object of principal conſequence.

20. The conſtruction of fig. 2. remaining, through the points F and I (fig. 14.) draw the great circle FI. Biſect FI in Q, and through the points K and Q draw the arc KQ, which will be perpendicular to IF. To determine by computation the arc ED which meaſures the angle ſubtended by the obſerved objects, three ſpherical triangles, KQF or KIF, IFB, and DBE, muſt be ſolved, for which the data are evidently ſufficient;

or the value of ED may be obtained from the ſolution of two triangles KQF and DFL, with the proportion demonſtrated in art. 9.

21. To proceed with the computation, through D draw the arc DL perpendicular to FI, and let the ſine of QKF = p, being the ſine of half the arc OP, the meaſure of IKF: put the ſine of KF = s, the ſine of DF = m, the ſine of DFK = n, radius = 1. In the right-angled ſpherical triangle KQF, the properties of ſpherics give this proportion: as radius to the ſine of KF ſo is the ſine of QKF to ſine of QF; wherefore ſin. QF = sp; cof. QF = $\sqrt{1-s^2p^2}$; and ſin. FI (FI being double to QF) = $2sp \times \sqrt{1-s^2p^2}$. Moreover, becauſe as rad. to cof. QK ſo is cof. QF to cof. KF, we have cof. KQ = $\frac{\sqrt{1-s^2}}{\sqrt{1-p^2s^2}}$; and ſin. KQ = $\frac{\sqrt{1^2-s^2p^2}}{\sqrt{1-s^2p^2}}$. And ſince as rad. : ſin. QFK ſo is ſin. KF to ſin. KQ; this proportion gives ſin. QFK = $\frac{\sqrt{1-p^2}}{\sqrt{1-s^2p^2}}$; and becauſe the ſine of the angle LFD is the ſine of the difference (or ſum) of the angles QFK, DFK, of which the ſines are, ſin. QFK = $\frac{\sqrt{1-p^2}}{\sqrt{1-s^2p^2}}$ juſt found, and ſin. DFK = n by the data, we have from the rules of trigonometry,

$$\text{ſin. } * \text{ DFL} = \frac{\sqrt{1-p^2} \times \sqrt{1-n^2} \mp \sqrt{p^2n^2-n^2s^2p^2}}{\sqrt{1-s^2p^2}},$$

and ſince in the right-angled triangle LDF, as rad. : ſin. DF :: ſin. DFL : ſin. DL, and by the problem ſin. DF = m it appears, that

* If the points P and Q be on different * ſides of the point O as they are repreſented in the conſtruction, the laſt term will be affected with the ſign —: if P and Q be on the ſame ſide of O, the ſign of the laſt term will be +. It may be here obſerved, concerning the geometrical conſtruction (fig. 2. and 3.) that when P and Q are on different ſides of O, the angle obſerved ED will be greater than when thoſe points are on the ſame ſide of the initial point O, the arcs OP, Op, being equal.

* Compare fig. 2.

$$\text{ſin. DL} = \frac{\sqrt{1-p^2} \times \sqrt{m^2-m^2n^2} \mp \sqrt{p^2m^2n^2-p^2m^4n^2}}{\sqrt{1-s^2p^2}}, \text{ and}$$

$$\overline{\text{ſin. DL}}^2 = \frac{m^2-m^2n^2-p^2m^2+2p^2m^2n^2-m^2n^2p^2s^2 \mp 2m^2pn \times \sqrt{1-p^2} \times \sqrt{1-s^2} \times \sqrt{1-n^2}}{1-s^2p^2}$$

and the ſquare of the coſine of DL

$$= \frac{1-s^2p^2-m^2+m^2s^2+p^2m^2-2p^2m^2n^2+m^2n^2p^2s^2 \pm 2m^2pn \times \sqrt{1-p^2} \times \sqrt{1-s^2} \times \sqrt{1-n^2}}{1-s^2p^2}$$

The ſine of IF was ſhewn to be $2sp \times \sqrt{1-s^2p^2}$, and its ſquare $= 4s^2p^2 \times \overline{1-s^2p^2}$: moreover, it was demonſtrated in art. 9. that as $\overline{\text{rad.}}^2 : \overline{\text{coſ. DL}}^2 :: \overline{\text{ſin. IF}}^2 : \overline{\text{ſin. } \tfrac{1}{2}\text{ED}}^2$ which gives, by ſubſtituting the values of $\overline{\text{coſ. DL}}^2$ and $\overline{\text{ſin. IF}}^2$, and multiplying the $\overline{\text{coſ. DL}}^2$ into $\overline{\text{ſin. IF}}^2$, $\overline{\text{ſin. } \tfrac{1}{2}\text{ED}}^2 =$

$$4s^2p^2 \times 1-s^2p^2-m^2+m^2n^2+p^2m^2-2p^2m^2n^2+m^2n^2s^2p^2 \pm 2m^2pn \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2}$$

and the coſine of $\tfrac{1}{2}\text{ED}^2 =$

$$1-4s^2p^2 \times 1-s^2p^2-m^2+m^2n^2+p^2m^2-2p^2m^2n^2+m^2n^2s^2p^2 \pm 2m^2pn \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2}$$

finally the coſine of ED is therefore $=$

$$1-8s^2p^2 \times 1-s^2p^2-m^2+m^2n^2+p^2m^2-2p^2m^2n^2+m^2n^2s^2p^2 \pm 2m^2pn \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2}.$$

22. The particular caſes inferred from the geometrical conſtruction may be compared with this analytical value of the coſine of ED, or of the angle ſubtended by the obſerved objects. If $s=1$ and $n=1$, by ſubſtituting 1 for s and n in the expreſſion juſt found, we ſhall have the coſine of $\text{ED} = 1-8p^2+8p^4$, which is the coſine of an arc four times greater than that of which the ſine $= p$. This anſwers to the properties of HADLEY's inſtrument, in which KF or the inclination of the reflecting planes to the plane of motion is 90°, and its ſine $= 1 = s$: moreover, in HADLEY's inſtrument, the fixed plane of reflection at the unmoved ſpeculum is parallel to the plane of motion, and therefore perpendicular to any ſecondary of that plane; its inclination to any ſecondary

condary therefore will be 90°, and the ſine of this inclination $= 1 = n$ by the problem. And ſince p is the ſign of half the inclination of the reflectors, the angle of which the coſine is $1 - 8p^2 + 8p^4$ will be twice the inclination of the reflecting planes, which is a property of HADLEY's inſtrument. In the analytical value of the coſine of ED, the laſt term is affected by two ſigns; theſe depend on the poſition of the ſecondary KP and the interſection Q in reſpect of the point O. If the ſecondary KP or the index CP be on the ſame ſide of O with the interſection Q (fig. 2.), the ſign of the laſt term is negative: if CP and Q be on oppoſite ſides of O, the ſign of the laſt term will be poſitive; and when DFK $= o$ or 180°, the whole term vaniſhes, becauſe in that caſe $n = o$. Alſo, if $m = o$, $n = 1$, $s = 1$, or if $p = 1$, the laſt term vaniſhes. When $m = s = \frac{1}{\sqrt{2}}$, KF $= 45°$: in this caſe, if $n = o$ the conſtruction will be that deſcribed in art. 15. and the coſine of the obſerved angle ED will equal $1 - 2p^2$, the other terms vaniſhing: and becauſe $1 - 2p^2$ is the coſine of an arc double to that of which the ſine $= p$, it follows, that the angle obſerved will be equal to the arc deſcribed by the index from o, of which the ſine of one half is by the problem $= p$. In every caſe, when $n = o$, that is, when the fixed plane of reflection at the unmoved ſpeculum coincides with the primitive ſecondary KO (fig. 2. and 12.), the coſine of ED $= 1 - 8s^2p^2 \times 1 - s^2p^2 - m^2 + p^2m^2$.

23. The ſine of ED will be neceſſary (art. 27.) to aſcertain the variation of ED from the truth occaſioned by errors in the data; to obtain ſin. ED let

$1 - s^2p^2 - m^2 + m^2n^2 + p^2m^2 - 2p^2m^2n^2 + m^2n^2s^2p^2 \pm 2m^2np \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2} = d$: then (art. 21.) from the value of $\overline{\text{col. } \frac{1}{2}\text{ED}}^2$ we have ſin. ED $= 4sp \times \sqrt{d} \times \sqrt{1 - 4s^2p^2d}$. When s is very ſmall,

and

and $n=o$, $d=1-m^2+m^2p^2$ nearly, which gives

$$\text{ſin. ED}=45p\times\sqrt{1-m^2+m^2p^2} \text{ nearly.}$$

24. The coſine of the obſerved angle repreſented by ED (fig. 2. and 14.) in the conſtruction, being computed from the four given quantities p, s, m, and n, if either of theſe ſhould deviate from its true value, the angle deduced will be erroneous: and from the general expreſſion for the coſine of ED, an eſtimation of this error will be obtained. In the inveſtigation, however, it muſt be obſerved, that although the ſmall increments or decrements of arcs or ſines are aſſumed proportional to the fluxions of theſe quantities, which is ſtrictly true only in the naſcent ſtate of the increments or decrements, yet when the given variations are in a practical ſenſe very ſmall, the eſtimation of correſponding variations will be in general ſufficiently exact for practical purpoſes.

25. Small increments and decrements, that is, ſmall variations, being aſſumed proportional to the fluxions of arcs and of their ſines and coſines, if the variation of the ſine or coſine of any given arc be known, the cotemporary variation of the arc will be for the moſt part inferred from the following proportions: as ſin. : rad. :: $-\dot{\text{cof.}}$: $\dot{\text{arc}}$; and as coſ. : rad. :: $\dot{\text{ſin.}}$: $\dot{\text{arc.}}$ But theſe proportions muſt be uſed under reſtrictions very neceſſary to be inſerted in this place, being true when applied to the intermediate parts of the quadrant only and failing at the extremities; for example, at the very beginning of the quadrant, or at the very end of the ſemi-circle, the variation of the coſine is the verſed ſine of the arcs increment or decrement, which gives the proportion as ſin. : 2 × rad. :: $-\dot{\text{cof.}}$: $\dot{\text{arc}}$, being wholly different from the former: in like manner, at the very extremity of the quadrant,

drant, the increment of the fine becomes the verſed fine of the arcs laſt increment, which gives this proportion: as cof. : 2 × rad. :: $\dot{\text{fin.}}$: $\dot{\text{arc.}}$ And ſince in this caſe $\dot{\text{arc}}$ = coſine, we ſhall have, $\dot{\text{arc}} = \sqrt{2 \times \dot{\text{fin.}}}$ radius being = 1. In the other parts of the quadrant which are not very near its extremity, $\dot{\text{arc}} = \frac{\dot{\text{fin.}}}{\text{cof.}}$; having given, therefore, the variation of the fine or coſine of any arc, the fine or coſine being known, the cotemporary variation of the arc itſelf may be obtained, when it is either at the very extremities of the quadrant, or at ſome diſtance from thoſe extremities. The difficulty lies in aſcertaining in what part of the quadrant the value of the $\dot{\text{arc}} = \frac{\dot{\text{fin.}}}{\text{cof.}} = \frac{-\dot{\text{cof.}}}{\text{fin.}}$ begin to fail, and the value expreſſed by $\dot{\text{arc}} = \sqrt{2 \times \dot{\text{fin.}}}$ or $-\sqrt{2 \times \dot{\text{cof.}}}$ to take place. This leads to a general propoſition comprehending both theſe values for the arc's variation, extended to every part of the quadrant.

The propoſition is this: the difference of the coſines is to the chord of the difference of any two arcs, as the fine of an arithmetical mean between them to radius; and the difference of the ſines is to the chord of the difference, as the coſine of the ſame arithmetical mean to radius. Let AB, AF (fig. 15.) be the given arcs; BF their difference; BL, FH, the ſines; CL, CH, the coſines of the arcs AB, AF, reſpectively; join CA, CB, CF, and FB; FB will be the chord of the difference of the arcs AF, AB. Through B draw BG parallel to CA; then HL = BG will be the difference of the coſines, and FG the difference of the ſines. Biſect FB in D, ſo ſhall DA be an arithmetical mean between the arcs FA, BA; join DC, which will interſect FB at right angles in E: through D and E draw DK, EI, perpendicular

to

to CA : DK will be the ſine, and CK the coſine of the mean arithmetical DA : the ſimilar triangles CEI, CDK, FGB, give the following proportions :

HL or GB : FB :: DK : DC, and

GF : FB :: CK : DC, which was the propoſition to be demonſtrated *.

* When FB (fig. 15) is ſo ſmall in compariſon of FA, that FG ſhall be evaneſcent in compariſon of FH, FH and BL will be in the ratio of equality, and conſequently the ratio FH : FC equal to the ratio BL : BC, or to the ratio DK : DC ; for this reaſon, and becauſe it has been proved, that as HL : FB :: DK : DC, it follows, that as HL : FB :: FH or BL : BC, that is, as the variation of the coſine is to cotemporary variation of the arc, ſo is the ſine of the varying arc to radius ; and, for ſimilar reaſons, as the variation of the ſine is to the cotemporary variation of the arc, ſo is the coſine to radius.

If BA be ſo diminiſhed that FG ſhall bear a finite proportion to FH, and too great to be neglected, BL will not be either to FH or to DK in a ratio of equality : conſequently, FH or BL muſt no longer be ſubſtituted for DK : as BA becomes leſs, FB being ſtill ſuppoſed evaneſcent, DK approximates to the ſine of ½FB to which it is ultimately equal when B and F arecoinciding with A (fig. 16.). In which caſe the proportion will become as HL or HA : FB or FA :: ½FA : to CA, that is, as the verſed ſine of FA is to the arc FA ſo is half the arc FA to radius, or ſo is the arc FA to diameter.

The propoſitions which have been demonſtrated, comprehend the variation of the arc expreſſed in terms of the cotemporary variation of the ſine or coſine in every part of the quadrant without limitation, it being only allowed to ſubſtitute the arc FB inſtead of its chord, theſe quantities approximating the more nearly to equality as FB is ſmaller, and being ultimately equal in their evaneſcent ſtate. Moreover, it will be eaſy from what has preceded to conſtruct a plane right-lined triangle, which ſhall be ſimilar to the mixtilinear triangle contained under an arc, its ſine and verſed ſine when they are diminiſhed ſine limite. Let FA (fig. 16.) be any arc, FA the chord, FH the ſine, CH the coſine of the arc FA. Biſect FA in D, join CD, and draw the right ſine DK : then will the plane right-lined triangle KDC continually approximate to ſimilarity with the mixtilinear triangle FDAH as FA becomes ſmaller, and the two triangles will be ultimately ſimilar when FA is vaniſhing.

From theſe geometrical proportions, having given any arc and the variation of its ſine or coſine, the cotemporary variation of the arc may be eſtimated by computation in general for any part of the quadrant. Let the ſine of any arc be s, the coſine $= c$, the chord of the arc's variation $= x$, the given variation of the coſine $= d$, or the given variation of the ſine $= b$, radius $= 1$; then if the coſine of the arc increaſes by the difference d, the chord of the cotemporary decreaſe of the arc, or

$$-x = \sqrt{2s^2 - 2dc \mp \sqrt{2s^4 - 2dc^3 - 4d^2}}$$

and if the ſine of the given arc increaſes by the difference b

$$+x = \sqrt{2c^2 - 2bs \mp \sqrt{2c^4 - 2bs^3 - 4b^2}},$$

which are the mathematically true values of the chord FB, and will approximate to the magnitude of the arc FB as that arc is continually diminiſhed. The following expreſſions for the chord of the variation x are more compendious, and will be ſufficiently near the truth when FB is very ſmall.

$$-x = \frac{s}{c} - \sqrt{\frac{s^2}{c^2} - \frac{2d}{c}}$$

$$+x = \frac{c}{s} - \sqrt{\frac{c^2}{s^2} - \frac{2b}{s}}.$$

In theſe four expreſſions it muſt be obſerved, that the ſine and coſine are ſuppoſed to vary by increaſe: ſhould the variation be a decrement, the ſign of x and of b or d muſt be changed.

26. Let the quantities p, s, m, n, vary by ſmall increments $\dot{p}$, $\dot{s}$, $\dot{m}$, $\dot{n}$, reſpectively, then to obtain the cotemporary variation of coſ. ED, becauſe (art. 21.)

$$\text{Coſ. ED} = 1 - 8s^2p^2 \times \overline{1 - s^2p^2 - m^2 + m^2n^2 + m^2p^2 - 2p^2m^2n^2 + m^2n^2s^2p^2} \mp 2m^2np \times \sqrt{1 - p^2} \times \sqrt{1 - n^2} \times \sqrt{1 - s^2},$$

by taking the fluxion of the equation we have

$$\dot{\text{col. ED}} = -16\dot{s}^2p^2 \times \overline{1-2s^2p^2-m^2+n^2m^2+2m^2p^2-2n^2m^2p^2+2n^2m^2s^2p^2}$$
$$\pm \frac{mnp \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times 3-4p^2}{\sqrt{1-p^2}}$$
$$-16\dot{s}^2s^2 \times \overline{1-2s^2p^2-m^2+n^2m^2+p^2m^2-2p^2m^2n^2+2p^2m^2n^2s^2}$$
$$\pm \frac{m^2np \times \sqrt{1-n^2} \times \sqrt{1-p^2} \times 2-2s^2}{\sqrt{1-s^2}}$$
$$+16\dot{s}^2p^2mn \times \overline{1-n^2-p^2+2p^2n^2-s^2p^2n^2} \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}$$
$$+16\dot{s}^2p^2m^2n^2 \times -n+2p^2n-s^2p^2n \mp \frac{\sqrt{1-s^2} \times \sqrt{1-p^2} \times p \times \overline{1-2n^2}}{\sqrt{1-n^2}}.$$

27. This value of $\dot{\text{col. ED}}$ is expreſſed in terms of the variation of the ſines of the given quantities: if it be neceſſary to expreſs col. ED in terms of the variation of the arcs themſelves, it muſt firſt be conſidered to what part of the quadrant they belong: for example, if s be a ſine of an arc b not very near the extremity of the quadrant, and the variation be $\dot{s}$, the cotemporary variation of the arc b will be $\frac{\dot{s}}{\sqrt{1-s^2}}$; but if the variable arc be nearly $= 90°$, and becomes exactly equal to it ultimately having varied by a ſmall arc b of which the verſed ſine $= v$; then will $-\dot{s} =$ the verſed ſine of $\dot{b}$ and $-\dot{b} = \sqrt{2v}$. Laſtly, if the variable angle approximates to 90°, but is not equal to it, and the variation of its ſine ſhould be $= \dot{s}$, the cotemporary variation of the arc muſt be obtained from the general theorem in art 25. When either of the two latter caſes happen, the variation of the arc muſt be determined for each particular caſe; but it will be neceſſary to give a general expreſſion for $\dot{\text{col. ED}}$ in terms of the variations of the given arcs, of which p, s, m, n, are the reſpective ſines when theſe arcs are at ſome diſtance from 90°; this is contained in the next article.

28. Let the angle QKF $= a$ (fig. 14.); the arc KF $= b$; the arc DF $= c$, and the angle DFK $= d$; their reſpective increments being $\dot{a}$, $\dot{b}$, $\dot{c}$, and $\dot{d}$, their ſines p, s, m, and n, and the cotemporary increments of their ſines $\dot{p}$, $\dot{s}$, $\dot{m}$, and $\dot{n}$: from the proportion contained in art. 24. we ſhall have $\dot{p} = \dot{a} \times \sqrt{1-p^2}$, $\dot{s} = \dot{b} \times \sqrt{1-s^2}$, $\dot{m} = \dot{c} \times \sqrt{1-m^2}$, and $\dot{n} = \dot{d} \times \sqrt{1-n^2}$, which being ſubſtituted in the value of $\dot{\text{col. ED}}$ laſt found will give

$$\dot{\text{Col. ED}} = -16s^2 \times \sqrt{1-p^2} \times \dot{p}a \times 1 - 2s^2p^2 - m^2 + n^2m^2 + 2m^2p^2 - 4m^2n^2p^2 + 2s^2m^2n^2p^2 \pm \frac{m^2np \times \sqrt{1-s^2} \times \sqrt{1-n^2} \times 3-4p^2}{\sqrt{1-p^2}}$$

$$-16p^2 \times \sqrt{1-s^2} \times \dot{b} \times 1 - 2s^2p^2 - m^2 + n^2m^2 + p^2m^2 - 2p^2m^2n^2 + 2s^2m^2n^2p^2 \pm \frac{m^2np \times \sqrt{1-n^2} \times \sqrt{1-p^2} \times 2-3s^2}{\sqrt{1-s^2}}$$

$$+16s^2p^2 \times \sqrt{1-m^2}\dot{m}c \times 1-n^2-p^2+2p^2n^2-s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}$$

$$+16s^2p^2m^2 \times \sqrt{1-n^2} \times \dot{d} \times -n+2p^2n-s^2p^2n \mp \frac{p \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times 1-2n^2}{\sqrt{1-n^2}}.$$

This quantity (art. 23.) being divided by the ſine of the obſerved angle, the variation of that angle or $\dot{\text{ED}}$ will be the quotient.

29. In the expreſſion for $\dot{\text{col. ED}}$ contained in art. 26. the variations $\dot{p}$, $\dot{s}$, $\dot{m}$, and $\dot{n}$, are arbitrary, as are $\dot{a}$, $\dot{b}$, $\dot{c}$, and $\dot{d}$, in the laſt article. If a condition be annexed to the variation of any of them, two or more may become dependant on each other; and their relation muſt be determined by the nature of the caſe. Moreover, if one or more of the given arcs and their ſines ſhould be correct, the variations correſponding and all the terms multiplied into them will vaniſh. To give an example of the uſe of theſe expreſſions before they are applied to the immediate purpoſe of examining the new conſtructions

deſcribed in art. 15. and 16. let it be required to aſſign what error is occaſioned in obſerving a given angle with a HADLEY's ſectant, in which the teleſcope is parallel to the plane of motion, but the two reflectors deviate from their perpendicular to that plane by a ſmall angle $\dot{b}$. Suppoſe the error of half the arc pointed to by the index to be $\dot{a}$, and conſequently the error of the ſine of half that * arc $= \dot{a} \times \sqrt{1-p^2} = \dot{p}$: in this caſe, becauſe the inclination of the reflectors to the plane of motion is nearly $= 90°$, the variation of the ſine will be equal to the verſed ſine of the ſmall arc $\dot{b}$, by which the inclination deviates from 90°; let v be the verſed ſine of $\dot{b}$, then will $-\dot{s} = v$ (s varying by a decrement of v). Moreover, becauſe a condition is annexed, which is, that the line of obſervation is parallel to the plane of motion, the variations $\dot{s}$, $\dot{m}$, and $\dot{n}$, will be dependent on each other. To inveſtigate their relation let FO $= \dot{b}$ (fig. 7.) be the ſmall arc which meaſures the deviation of the reflectors from the perpendicular to the plane of motion: then, becauſe ſin. DO $= m$, and ſin. DOF $= n = 1$ by the problem, when F from having been coincident with O has moved through the arc OF, it is plain, that $\dot{n}$ = ſin. DFO − ſin. DOF = ($\dot{n}$ being a decrement); but, by the † properties of ſpherics, cof. DFO $= \frac{\sqrt{2v} \times \text{cof. DF}}{\text{ſin. DF}} = \frac{\sqrt{2v} \times \sqrt{1-m^2}}{m}$: and FO being very ſmall, the

* p here, as in the general ſolution, denotes the ſine of half the arc to which the index on the plane of motion is directed, that is, p = the ſine of one-fourth of the angle obſerved in Mr. HADLEY's conſtruction.

† Fig. 7. as rad. : cotang. DF :: tang. FO : cof. DFO, that is, FO being very ſmall, and therefore $\text{FO}^2 = 2 \times$ verſed ſine of FO, as rad. : cotang. DF :: $\sqrt{2v}$: cof. DFO : by the problem ſin. DF $= m$, and cof. DF $= \sqrt{1-m^2}$, wherefore cot. DF $= \frac{\sqrt{1-m^2}}{m}$, which gives cof. DFO $= \sqrt{2v} \times \frac{\sqrt{1-m^2}}{m}$.

ſine

ſine of DFO $= 1 - \frac{v \times \overline{1-m^2}}{m^2}$, from which $1 = \text{ſin. DOF}$ being ſubtracted leaves $\dot{n} = \frac{-v \times \overline{1-m^2}}{m^2}$; or becauſe $\dot{s} = -v$, $\dot{n} = \frac{+\dot{s} \times \overline{1-m^2}}{m^2}$. Moreover, the *quantity $\frac{1-s^2}{1-n^2}$ in the naſcent ſtate of $\overline{1-s^2}$ and $\overline{1-n^2} = \frac{m^2}{1-m^2}$: and $\sqrt{\frac{1-s^2}{1-n^2}} = \frac{m}{\sqrt{1-m^2}}$. Making therefore in the general expreſſion contained in art. 26. $\frac{-v \times \overline{1-m^2}}{m^2} = \dot{n}$, $-v = \dot{s}$, $\dot{a} \times \sqrt{1-p^2} = \dot{p}$, and $\frac{m}{\sqrt{1-m^2}} = \sqrt{\frac{1-s^2}{1-n^2}}$, $n = s = 1$, we ſhall have $\dot{\text{Col.ED}} = -16p\dot{a} \times \sqrt{1-p^2} \times \overline{1-2p^2} + 16p^2 v \times \overline{1-2p^2} + p^2 m^2 \mp mp \times \sqrt{1-m^2} \times \sqrt{1-p^2}$ $+ 16p^2 v \times \overline{1-p^2-m^2+m^2p^2 \mp mp \times \sqrt{1-m^2} \times \sqrt{1-p^2}} = -16p\dot{a} \times \sqrt{1-p^2} \times \overline{1-2p^2} +$ $16p^2 v \times 2 - 3p^2 - m^2 + 2p^2 m^2 \mp 2mp \times \sqrt{1-m^2} \times \sqrt{1-p^2}$: and becauſe the ſine of the obſerved angle is $4p \times \sqrt{1-p^2} \times \overline{1-2p^2}$, the error of the obſervation itſelf, that is, $\dot{\text{ED}} = 4\dot{a} - \frac{4pv \times 2 - 3p^2 - m^2 + 2p^2 m^2 \mp 2mp \times \sqrt{1-m^2} \times \sqrt{1-p^2}}{\sqrt{1-p^2} \times \overline{1-2p^2}}$.

In this example the poſition of the teleſcope has been ſuppoſed exactly in the plane of motion; ſhould it be inclined to that plane at a ſmall angle, of which the verſed ſine $= v$, the poſition of the reflectors and the arc pointed to by the index being correct, the general value of $\dot{\text{col. ED}}$ will give the error of the obſervation, or $\dot{\text{ED}} = \frac{-4pv \times \sqrt{1-p^2}}{\overline{1-2p^2}}$ †.

30. To

* The naſcent value of $\frac{1-s^2}{1-n^2} = \frac{-2s\dot{s}}{-2n\dot{n}}$; but $\dot{n} = \frac{-v \times \overline{1-m^2}}{m^2} = \frac{+\dot{s} \times \overline{1-m^2}}{m^2}$: wherefore $\frac{1-s^2}{1-n^2} = \frac{m^2}{1-m^2}$, when s and n are nearly $= 1$.

† When the poſition of the teleſcope only is erroneous, the points F and O coincide

30. To examine in what degree an obſervation taken by the new conſtruction deſcribed in art. 15. is affected by known errors in the given quantities, let the reflectors B and C deviate by exceſs from their true angle of inclination to the plane of motion by a ſmall angle $+\dot{b}$: let the angle of incidence on the fixed ſpeculum be too great by the increment $\dot{c}$: let the fixed plane of reflection deviate from the ſecondary KO with which it ſhould coincide by a ſmall angle $\dot{d}$; and laſtly, let the error of the arc pointed to by the index be $= 2\dot{a}$; then theſe variations are arbitrary, no condition being annexed. Moreover, by the conſtruction $m = s = \frac{1}{\sqrt{2}}$, and $n = o$, which values being ſubſtituted in the general expreſſion contained in art. 28. we ſhall have.

$\dot{\text{Cof.ED}} = -4\dot{a}p \times \sqrt{1-p^2} - 4p^2\dot{b} \times \overline{1-p^2} + 4p^2\dot{c} \times \overline{1-p^2} \mp 2\sqrt{2}p^3\dot{d} \times \sqrt{1-p^2}$;

and becauſe the ſine of the angle meaſured $= 2p \times \sqrt{1-p^2}$, the error of the obſervation required, or

coincide (fig. 7.) let the inclination of the teleſcope to the plane of motion with which it ſhould coincide, be meaſured by the ſmall arc Dd; then the correſponding variation of the angle DOK will be DOd. Let D$d = v$, and its verſed ſine $= v$; ſince the ſine of DO $= m$, and the ſine of DOK $= 1 = n$ by the problem, $\dot{n} =$ the verſed ſine of DOd; but DO$d = \frac{v}{m}$, and the verſed ſine of DO$d = \frac{v^2}{2m^2} = \frac{v}{m^2}$; wherefore $\dot{n} = \frac{-v}{m^2}$. This being premiſed, it appears from art. 26. when $\dot{p}$, and $\dot{s}$, are $= o$, that $\dot{\text{cof.}}\ \dot{\text{ED}} = +16s^3p^3m^3\dot{n} \times -n + 2p^3n - s^2p^3n, \pm \frac{\sqrt{1-s^2} \times \sqrt{1-s^2} \times \overline{1-2n^2}}{\sqrt{1-n^2}}$

$+16s^2p^2m\dot{m} \times 1 - n^2 - p^2 + 2p^2n^2 - s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}$

in which quantity, ſubſtituting 1 for s, 1 for n, and $\frac{-v}{m^2}$ for $\dot{n}$, we ſhall have $\dot{\text{cof.}}\ \dot{\text{ED}} = +16p^2v \times \overline{1-p^2}$, which being divided by the ſine of the obſerved angle $= 4p \times \sqrt{1-p^2} \times \overline{1-p}$, the quotient will be the variation of that angle or $\dot{\text{ED}} = \frac{-4vp \times \sqrt{1-p^2}}{1-2p^2}$.

ED =

$$\dot{\overline{ED}} = 2\dot{a} + 2p\dot{b} \times \sqrt{1-p^2} - 2p\dot{c} \times \sqrt{1-p^2} = \sqrt{2p^3\dot{d}}.$$

It appears from the firſt term $2\dot{a}$, that an error in the arc pointed to by the index, cauſes an equal error in the obſerved angle; whereas a double error is cauſed by it in HADLEY's ſextant, which gives the new conſtruction conſiderable advantages: to counter-balance theſe the errors in HADLEY's conſtruction, cauſed by a wrong poſition of the reflecting planes, &c. are almoſt evaneſcent; whereas the three laſt terms in the value of $\dot{\overline{ED}}$ juſt found, may become conſiderable, unleſs great care be taken in making the adjuſtments: the ſeparation alſo of the images, when in contact, cauſed by any unſteadineſs of the inſtrument will be greater than in Mr. HADLEY's conſtruction *; but

* The variation of the obſerved angle $\dot{\overline{ED}}$ will ſhew how much the images ſeen in contact in the field of the teleſcope will appear to divaricate on any motion of the entire conſtruction. For example, while the images of the obſerved objects are coincident in the field of the teleſcope, ſuppoſe that Mr. HADLEY's inſtrument were turned round in its own plane through a ſmall angle: here s, n, and p, not being affected by this motion, it follows, that $\dot{s}$, $\dot{n}$, and $\dot{p} = 0$, $m =$ the ſine of the angle of incidence on the fixed ſpeculum, being the only quantity which ſuffers alteration: let its variation $= \dot{m}$, which will give from art. 26. the correſponding variation in the coſine of the obſerved angle, or

$$\dot{\overline{\text{Cof. ED}}} = +16s^2p^2mm \times 1-n^2-p^2+2p^2n^2-s^2p^2n^2 = 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}$$

$= 0$, becauſe $n = s = 1$. Wherefore any motion of the images in the plane of the inſtrument will not cauſe the leaſt ſeparation of them. Now ſuppoſe the whole to be turned on an axis ſituated in the plane of motion, and perpendicular to the teleſcope's axis: if the angular motion be meaſured by an arc of which the verſed ſine $= v$, the points in contact will be ſeparated through an angle $= \frac{4vp \times \sqrt{1-p^2}}{1-2p^2}$, p being the ſine of one quarter of the angle obſerved; but the quantity v being very ſmall, when the angular motion does not exceed 30′, the divarication of the images will be inconſiderable. All oblique motions of the teleſcope's axis, and conſequently of the image ſeen by direct rays, may be reſolved into thoſe that have

but in meaſuring the ſmaller angles, this ſeparation of the images, as well as the errors expreſſed by the three laſt terms will be greatly diminiſhed while that which is denoted by $2a$ contained in the firſt term is not increaſed. On the whole, from the properties which have been demonſtrated to belong to this conſtruction deſcribed in art. 15. it may ſeem worthy of attention in practice, for ſome aſtronomical as well as other uſes.

31. By the ſame way of examination it may be judged, whether the method of obſerving by two reflections from plane ſurfaces be applicable to the menſuration of ſmall angles, according to the conſtruction deſcribed in art. 16. Let the errors of the four given quantities (as in the laſt article) be $2a$ = the error of the arc

have been conſidered, which are perpendicular to each other: and from hence the reaſon appears, why the motion of a ſhip at ſea does not much diſturb the obſervation of angles by Mr. HADLEY's inſtrument.

The new conſtruction deſcribed in art. 15. is not ſo well adapted for obſervation where it cannot be ſteadily fixed. When the images are in contact, if the inſtrument be turned in its own plane through a ſmall angle ϵ'' the ſeparation of the images will be $= 2\epsilon'' p^2$ (becauſe $\delta'' = \sqrt{2}\,\epsilon''$, vid. p. 426.) p ſignifying the ſine of half the obſerved angle: this it is evident will moſt affect the obſervation of the larger angles; but in meaſuring thoſe that are ſmall, the divarication will become inconſiderable. Moreover, if the angular motion of the inſtrument be ϵ'', when it turns round an axis in the plane of motion, and perpendicular to the teleſcope's axis, the ſeparation of the images will be $= 2\epsilon'' p \times \sqrt{1-p^2}$, p. 426. which it is plain will moſt diſturb the obſervations of angles about 90°, but will ſcarcely alter the contact of the images, when the angles meaſured are very ſmall, or near 180°.

The objects obſerved and their images are here underſtood to be phyſical points: thus, when the two images of the ſun are ſeen by direct and reflected rays, and the limbs appear preciſely in contact, if by any motion of the inſtrument the contact is diſturbed, the points which before touched, being the obſerved objects, are ſaid to be ſeparated, whether the centres of the ſolar images approach or recede from each other, the ſeparation being eſtimated in the direction of an arc which paſſes through the centers of the two ſolar images.

Experience muſt determine in what degree this ſeparation of the images will diſturb obſervations taken at ſea with the new conſtruction.

pointed to by the index; $\dot{b}$ = the error of the inclination of the reflectors to the plane of motion; $\dot{c}$ = the error in the angle of incidence on the fixed ſpeculum; and $\dot{d}$ = the inclination of the fixed plane of reflection to the primitive ſecondary with which it ſhould coincide. Referring to the general value of $\overline{\text{col. ED}}$, (art. 28.) and making $n = o$ we ſhall have

$$\overline{\text{Col. ED}} = -1 \dot{6} \times \sqrt{1-p^2}\, p s^2 \times \overline{1-2s^2p^2-m^2+2m^2p^2} - 16p^2 s\dot{s} \times \sqrt{1-s^2} \times \overline{1-2p^2s^2-m^2+m^2p^2}$$
$$+ 16 s^2 p^2 \dot{c} m \times \sqrt{1-m^2} \times 1-p^2 = 16 m^2 p^2 s^2 \dot{d} \times \sqrt{1-s^2} \times \sqrt{1-p^2},$$

and becauſe s being very ſmall, the ſine of ED (art 23.) approximates to $45p \times \sqrt{1-m^2+p^2m^2}$, the error of the obſervation itſelf, or $\overline{\text{ED}} = \frac{45s \times \sqrt{1-p^2} \times \overline{1-2p^2s^2-m^2+2p^2m^2}}{\sqrt{1-m^2+p^2m^2}}$

$$+ \frac{45p\dot{b} \times \sqrt{1-s^2} \times \overline{1-2p^2s^2-m^2+m^2p^2}}{\sqrt{1-m^2+p^2m^2}}$$
$$- \frac{45pm\dot{c} \times \sqrt{1-m^2} \times 1-p^2}{\sqrt{1-m^2+p^2m^2}}$$
$$\pm \frac{45m^2p^2s\dot{d} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}{\sqrt{1-m^2+p^2m^2}},$$

The firſt term of this expreſſion gives the relation between any ſmall variation in the arc pointed to by the index, and the correſponding alteration in the angle obſerved; if therefore the variation on the divided arc be any ſmall angle $\pm 2\dot{a}$, + or − the firſt term will expreſs the variation by which the obſerved angle is increaſed or diminiſhed. According to the magnitude of m and s aſſumed for this conſtruction m being the ſine of 7°, and s = the ſine of 10′, it appears, that at the very beginning of the ſcale one ſecond of a degree in the angle obſerved correſponds to ſomewhat leſs than three minutes on the divided arc OP; that is, when $2\dot{a}$ = about 173″, $\overline{\text{ED}} = 1''$, $\dot{c}$, $\dot{b}$, and $\dot{d}$, not being here conſidered. When $p = \frac{1}{4}$, the index then pointing to 60°, one ſecond in the obſerved angle cor-

reſponds to about 199″ in the divided arc OP: when p is nearly $=1$, the index being then directed to almoſt 180°, it muſt deſcribe above 2 degrees to make an alteration of 1″ in the obſerved angle. The ſecond term expreſſes the variation in the obſervation occaſioned by an error $\dot{b}$ in adjuſting the inclination of the reflecting planes to the plane of motion; but $\dot{b}$ (art. 18.) cannot exceed $\frac{1}{4}$ of the leaſt angle viſible in the teleſcope, conſequently the utmoſt value of the ſecond term cannot be ſo great as that leaſt angle, being at its limit when $p=1$: it is manifeſt when p is ſmall, that the ſecond term is ſo much diminiſhed, as to be in a phyſical ſenſe evaneſcent. The ſame may be ſaid of the fourth term, containing the error of the optical adjuſtment $\dot{d}$, which beſides is multiplied into s the ſine of 10′. The third term is occaſioned by the error $\dot{c}$, for which, conſiderable latitude muſt be allowed, ſuppoſe 3′: to eſtimate the effect of this error on the obſervation, let a caſe be aſſumed: let the index be directed to 90° when an obſervation is taken for determining the angle ſubtended between two objects: then will $p=\frac{1}{\sqrt{2}}$; by ſubſtituting $\frac{1}{\sqrt{2}}$ for p, the ſine of 7° for m, the ſine of 10′ for s, and 180″ for $\dot{c}$ in the third term, we ſhall have, by computation, the value of that term, or the error in the obſervation occaſioned by this deviation of the angle of incidence from its true magnitude $=.''090$ not the tenth part of a ſecond. This is rather an unfavourable caſe, the variation being not much leſs than at its maximum when $p=\frac{1}{\sqrt{2}}$: if p is ſmall, or nearly $=1$, the * variation will be wholly inſenſible.

32.

* The variation is a maximum when $p=$ the ſine of 35° 12′; and conſequently the arc pointed to by the index $=70°\ 24'$. Subſtituting therefore the ſine of 35° 12′ for

32. From contemplating the value of the error $= \overline{ED}$, as expreſſed by the four terms juſt referred to, ſome further obſervations are ſuggeſted. It appears, that ſetting aſide the error of the ſcale expreſſed by the firſt term (the angles meaſured being here ſuppoſed leſs than 22′ 56″ 33‴) all the others become ſmaller as the quantity p, and conſequently the angle meaſured is diminiſhed, p being multiplied into each of the three laſt terms. This is a material circumſtance: for the ſame given error would affect the menſuration of the ſmaller angles in a greater proportion than the larger. Moreover, if $p = 1$, or approximates to that quantity, the index then pointing to an angle nearly $=$ to 180°, the firſt term and the two laſt terms become almoſt evaneſcent, not only from the circumſtances that have been conſidered, but from the quantities $\overline{1-p^2}$, $\sqrt{1-p^2}$, which are multiplied into them. This alſo the obſerver may avail himſelf of: for ſuppoſing he ſhould know, that the angle of incidence on the fixed ſpeculum, and the fixed plane of reflection, are imperfectly adjuſted; and even that the divided arc is incorrect, he may almoſt wholly avoid the errors which they would occaſion, by ſo adjuſting the inclination of the reflectors to the plane of motion, in reſpect of the angle meaſured, that

for p in the third term, (p. 429.) every thing elſe remaining, the maximum of variation in the obſerved angle on account of 3′ difference in the angle of incidence on the fixed ſpeculum will appear to be $=$ ″.098.

The error of an obſervation occaſioned by a variation $\dot{c}$ in the angle of incidence on the fixed ſpeculum, can never exceed the $\frac{1}{2\,1\,1\,7}$ part of $\dot{c}$, which is the maximum of error, the angle meaſured being $=$ 22′ 56″ 33‴, the index then pointing to 70° 24′. The error, cauſed by a ſmall angle $\dot{d}$, at which the fixed plane of reflection is inclined to the primitive ſecondary with which it ſhould coincide, cannot exceed the $\frac{1}{2\,1\,1\,1\,8}$ part of $\dot{d}$, which is its maximum when the angle meaſured is 32′ 33″ 3‴, the index then being directed to 109° 20′.

the index ſhall point to ſome degree near to 180°, which is done by making that inclination very little more than one-fourth of the angle to be obſerved.

33. The tables I. and II. are calculated for taking the diameters of the ſun and planets: the conſtruction being formed on the principles which have been explained (art. 16. 17. 18. 31. 32. fig. 12. 13.). The fixed plane of reflection is coincident with the primitive ſecondary, and conſequently $n = 0$: the common inclination of the reflectors to the plane of motion = 10′; and the conſtant angle of incidence on the fixed ſpeculum = 7°.

TABLE I.

Arc pointed to by the index.	Obſerved angle.	Diff.
	′ ″ ‴	″ ‴
1°	0 20 47	20 47
2°	0 41 34	20 47
3°	1 2 21	20 47
4°	1 23 8	20 46
5°	1 43 54	20 46
6°	2 4 40	20 45
7°	2 25 25	20 45
8°	2 46 10	20 44
9°	3 6 54	20 43
10°	3 27 37	

TABLE II.

Arc pointed to by the index.	Obſerved angle.	Diff.
	′ ″ ‴	″ ‴
100°	30 32 51	13 28
101°	30 46 19	13 20
102°	30 59 39	13 12
103°	31 12 51	13 3
104°	31 25 54	12 54
105°	31 38 48	12 46
106°	31 51 34	12 37
107°	32 4 11	12 28
108°	32 16 39	12 19
109°	32 28 58	12 11
110°	32 41 9	

The

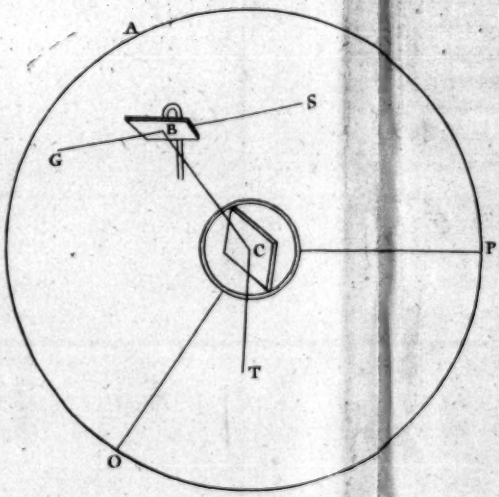
The divarication of the images while they traverſe the field of the teleſcope during the time of an obſervation, and the errors of the obſerved angle in conſequence of any change in the quantities m * and n (p. 429.) ſhould they appear to be of ſenſible magnitude, may be diminiſhed until they are in phyſical ſenſe evaneſcent, by altering the values of s and m (art. 31.): for this purpoſe it will be requiſite to uſe two ſeparate conſtructions; the one for obſerving very ſmall angles, thoſe, for example, which do not exceed 2′: and the other, for meaſuring ſuch angles as are ſubtended by the ſun and moon. In the former of theſe s may be aſſumed = the ſine of 30″, m = ſin. 7°, and $n = 0$. From theſe data tab. III. is calculated. In the other conſtruction, becauſe very ſmall reflecting ſurfaces are neceſſary to obſerve the ſun by two reflections (art. 17.) m may be aſſumed = ſin. 1°, s being = ſin. 10′, and $n = 0$: from which conditions the fourth table is calculated. Theſe tables may be eaſily extended, by calculating from the value of cof. ED or $\sqrt{\text{ſin. } \tfrac{1}{2}\text{ED}^2}$ contained in art. 21. or 22.

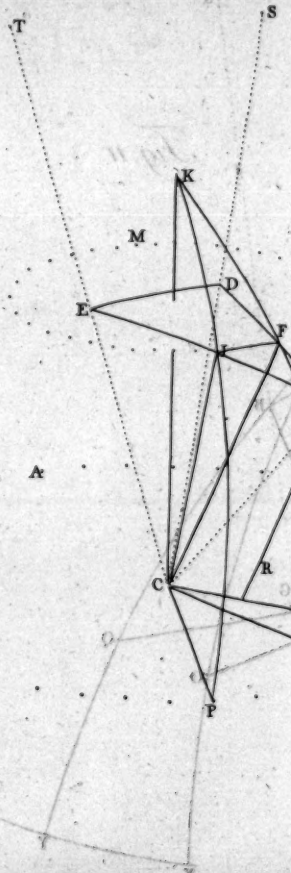
* Let the teleſcope with the entire conſtruction be ſteadily fixed: then if the objects obſerved be in contact while the touching points occupy the center of the field, the angle of incidence on the fixed ſpeculum will be of its true magnitude; that is, its ſine or m will = ſin. 7° in tables I. II. and III.; and m = ſin. 1° in table IV. The fixed plane of reflection alſo will be coincident with the primitive ſecondary in all the conſtructions correſponding to tables I. II. III. and IV. The teleſcope continuing unmoved, the diurnal motion of the heavens will cauſe the points in contact to leave the center of the field; this will occaſion no alteration in the quantities s and p, but will affect m and n only: therefore, $\dot{s}$ and $\dot{p} = 0$. To eſtimate the effects of this change in the values of m and n, let $\dot{m} = \dot{c} \times \sqrt{1 - m^2}$, and $\dot{n} = \dot{d} \times \sqrt{1 - n^2}$, as in art. 28. and ſuppoſe a line to be drawn through the centre of the field in a plane, perpendicular to the plane of motion: this line will be in the fixed plane of reflection; and any deviation of the points in contact through a ſmall angular ſpace $\dot{c}$, from the center of the field will cauſe an equal variation $\dot{c}$ in the

the angle of incidence on the fixed ſpeculum; the correſponding ſeparation of the images will be $= \frac{4\dot{s}pm\dot{c} \times \sqrt{1-m^2} \times \sqrt{1-p^2}}{\sqrt{1-m^2+m^2p^2}}$ (p. 429.) which is the greateſt poſſible when $p = \sqrt{\frac{\sqrt{9-10m^2+m^4}-3-3m^2}{4m^2}}$ = ſine of 35° 11′ 47″ (if m = ſin. 7°, and s = ſin. 10′, as in tab. I. and II.) being then $= \frac{\dot{c}}{1837}$; but if s = ſin. 30″, as in tab. III. the greateſt ſeparation will be only $\frac{\dot{c}}{36740}$, correſponding to leſs than one-third of a degree for the images motion through 10′ in the field of the teleſcope. In like manner let a line be drawn through the center of the field perpendicular to the line before deſcribed; any deviation of the points in contact through a ſmall angle $\dot{e}$ in the direction of this line will cauſe the plane of reflection at the fixed ſpeculum to be inclined to the primitive ſecondary with which it ſhould coincide at an angle $= \frac{\dot{e}}{m}$; this will occaſion a ſeparation of the images $= \frac{4mp^2s\dot{e} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}{\sqrt{1-m^2+m^2p^2}}$ (page 429. $\frac{\dot{e}}{m}$ being there ſubſtituted for $\dot{d}$) which is the greateſt poſſible when $p = \sqrt{\frac{\sqrt{9-8m^2}-3-4m^2}{4m^2}}$. If m = ſin. 7° and s = ſin. 10′, as in tab. I. and II. the greateſt ſeparation of the images will be $= \frac{\dot{e}}{1828} = \frac{\dot{d}}{14996}$; but if s = ſin. 30″ and m = ſin. 7°, as in tab. III. it will only $= \frac{\dot{e}}{36560} = \frac{\dot{d}}{299920}$, which anſwers to a divarication of leſs than 1‴ for the images motion through 10′ in the field of the teleſcope, in the direction of the line above deſcribed, which is drawn through the center of the field, and perpendicular to the fixed plane of reflection. It muſt be remembered, that the ſeparations of the images here eſtimated are greater than can poſſibly happen in theſe conſtructions, when the index is directed to any other points of the circumference of the plane of motion (the diſtance of the images from the center the field not exceeding 10′) and are, even in this caſe, phyſically ſpeaking, evaneſcent.

TABLE

Fig. 1.





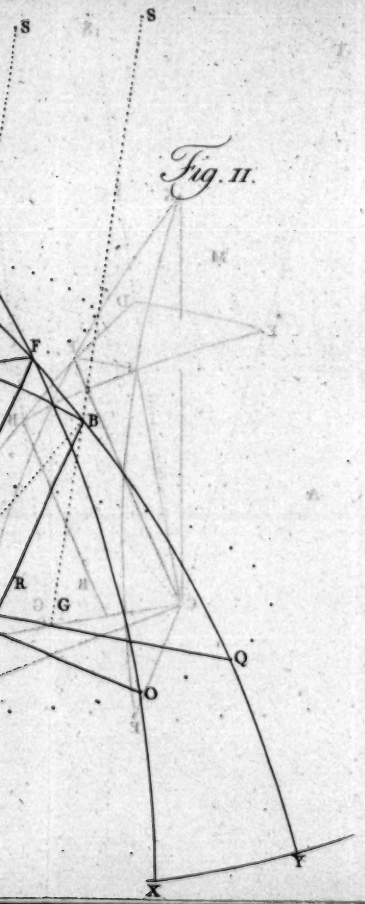


Fig. II.

Fig. III.

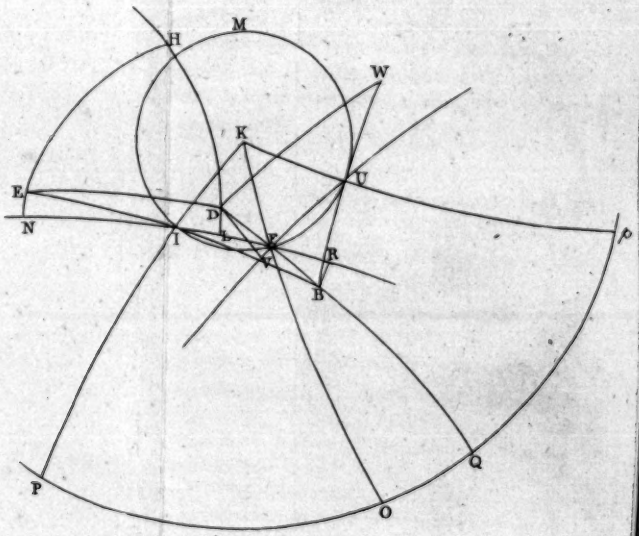


Fig. IV.

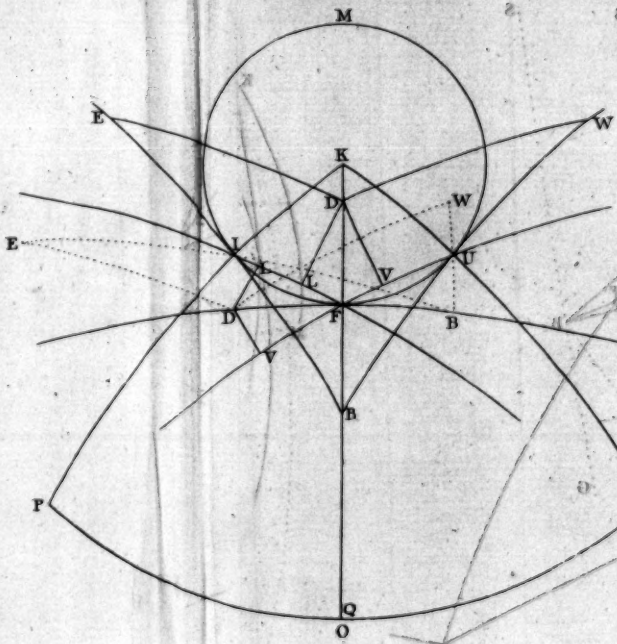
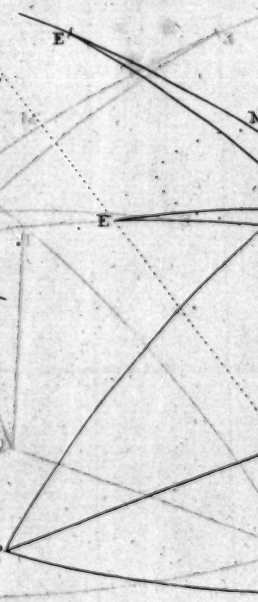


Fig. V.



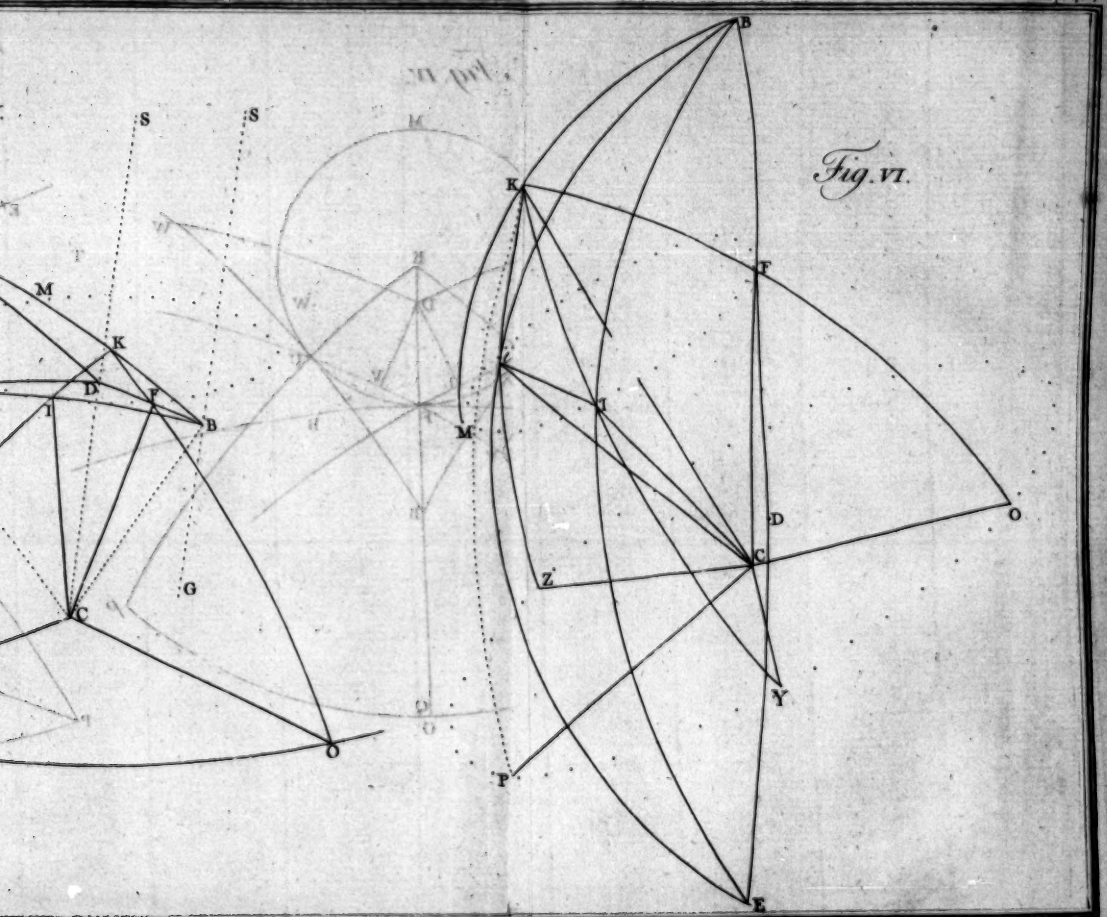


Fig. VI.

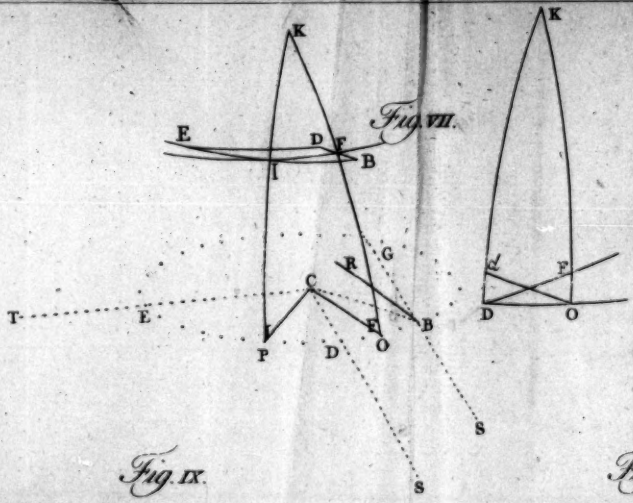


Fig. IX.

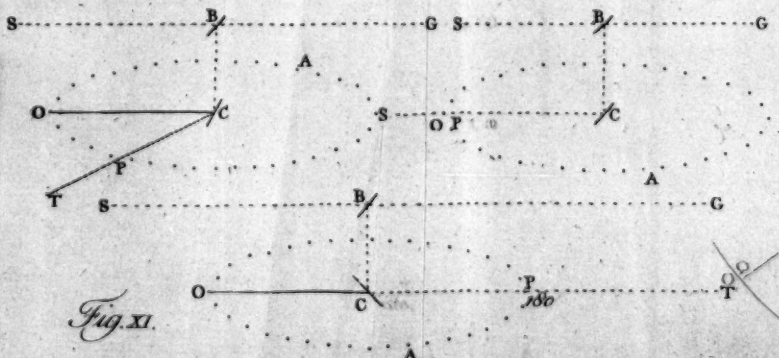


Fig. X.

Fig. XI.

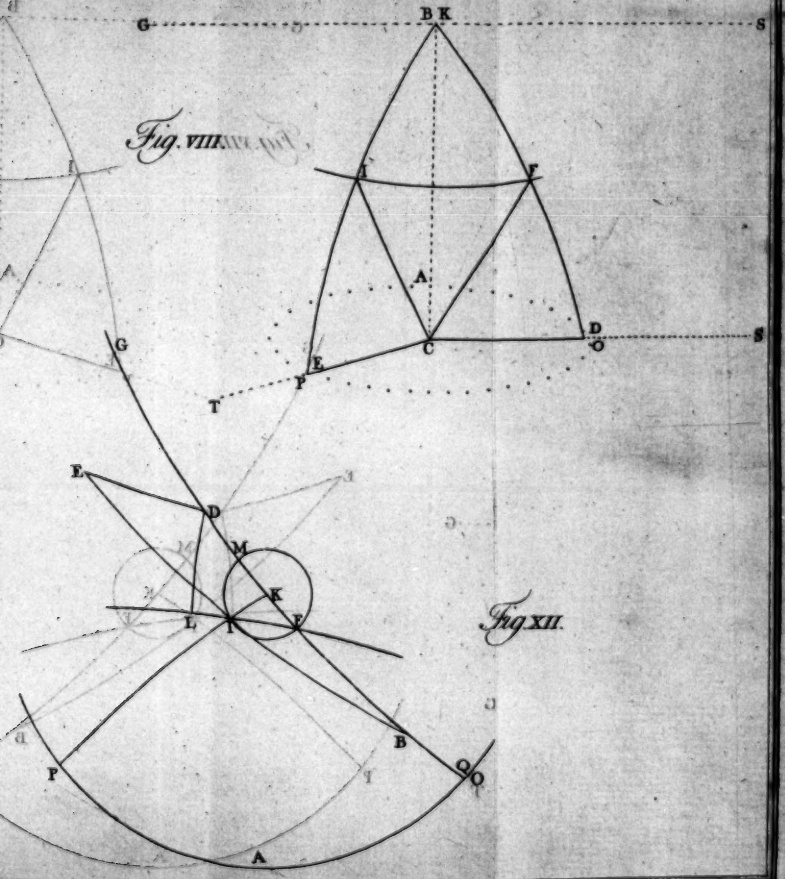


Fig. VIII.

Fig. XII.

Fig. XIII.

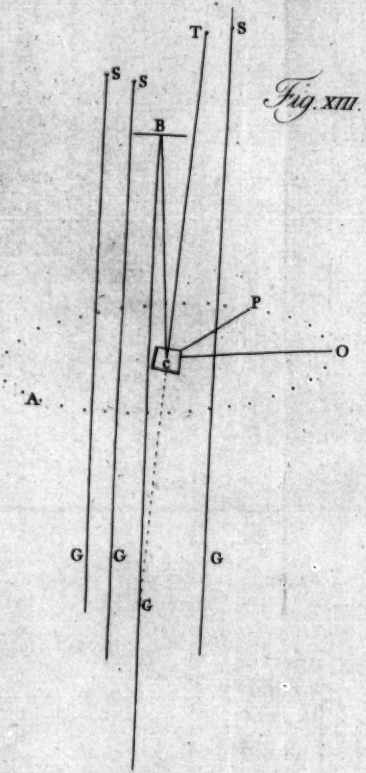


Fig. X

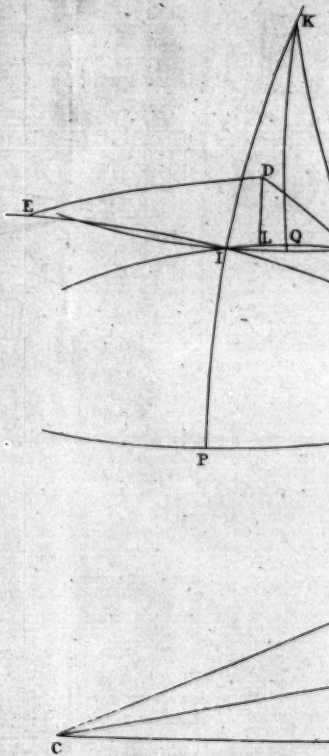


Fig. XIV.

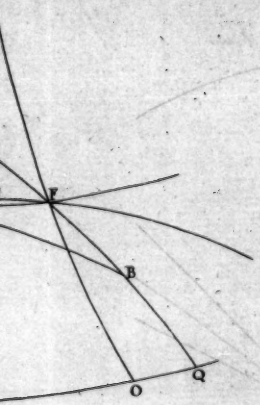
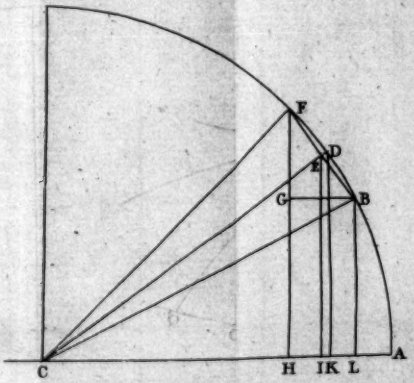


Fig. XV.



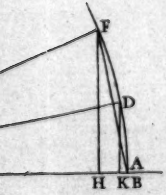


Fig. XVI.

TABLE III.

Arc pointed to by the index.	Observed angle.			Diff.
	°	′	″	‴
100°	1	31	39	40
101°	1	32	19	40
102°	1	32	59	40
103°	1	33	39	39
104°	1	34	18	38
105°	1	34	56	38
106°	1	35	34	38
107°	1	36	12	38
108°	1	36	50	37
109°	1	37	27	36
110°	1	38	3	

TABLE IV.

Arc pointed to by the index.	Observed angle.			Diff.	
	°	′	″	″	‴
100°	30	38	23	13	24
101°	30	51	47	13	15
102°	31	5	2	13	7
103°	31	18	9	12	58
104°	31	31	7	12	49
105°	31	43	56	12	41
106°	31	56	37	12	32
107°	32	9	9	12	23
108°	32	21	32	12	14
109°	32	33	46	12	6
110°	32	45	52		

XXVII. *An Account of the* Ophidium barbatum LINNEI. *By* P. M. Auguſtus Brouſſonet, *M. D.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read July 5, 1781.

THIS ſpecies of fiſh ſeems not to have been unknown to the ancients, though probably they confounded it with the *Conger*, to which it bears ſome reſemblance. Perhaps the early Greek and Latin writers on natural hiſtory have mentioned it under the name of *Tragus*, or *Callarias*; but for want of deſcriptions, they left us much in the dark concerning it. PLINY indeed ſpeaks of a fiſh which appears to be of this ſpecies: he calls it *Ophidion* *, and as that is the name given to it by all the modern writers, we are obliged to accept his ſynonymy without further inquiry.

The firſt author to whom we are indebted for a deſcription and figure of the *Ophidium* is BELLONIUS; yet it appears, that he was not certain of the name of this fiſh, ſince he calls it *Gryllus*, *falſo congrus*, *tragus*, *aſelli ſpecies*: nor was he leſs doubtful of the claſs to which he ſhould refer it, and therefore placed it among the *Aſelli*, or *Gadi*, though very different from the ſpecies of that family. RONDELETIUS, who wrote ſoon after BELLONIUS, has given us a better deſcription, and a more accurate figure of this fiſh, which he calls *Ophidion*,

* Ophidion or Ophidium, though the beſt is Ophidium, is not a good generic name, ſince it is a diminutive from Οφις.

with a reference to PLINY. In the figure of BELLONIUS the cirri are very ill repreſented, and the whole fiſh appears without any ſpots, whereas in the plate of RONDELETIUS it is covered with oblong ſpots. This remarkable difference between the figures of theſe authors was ſufficient to determine GESNERUS, and others who have written ſince their time, and who are to be conſidered rather as compilers than authors, to take the fiſh deſcribed by BELLONIUS to be a different ſpecies from that of RONDELETIUS.

WILLOUGHBY, who is the firſt ichthyologiſt who has given us any good deſcription of fiſh, treats largely of the *Ophidium*; and in his account deſcribes the ſcales, which are, as we ſhall hereafter explain, oblong, diſtinct, and diſpoſed without any regular order. This deſcription was ſufficient to aſcertain, that the difference between the figures aroſe from RONDELETIUS having drawn the ſcales omitted by BELLONIUS: yet the authors who wrote immediately after WILLOUGHBY, and particularly RAY in his Synopſis, follow GESNERUS, in maintaining two different ſpecies of *cirrata Ophidia*, one with, the other without, ſpots.

ARTEDI did not take notice of the ſpots; he deſcribes the fiſh in a genus to which he gives the name of *Ophidion*, and places that genus among the *Malacopterygii*. After him KLEINIUS once more took notice of the ſpots; but at the ſame time introduced another confuſion concerning this fiſh, ariſing from RONDELETIUS having ſaid, that it has two cirri, while WILLOUGHBY aſſerts it has four; but it is eaſy to reconcile theſe authors, for though the *Ophidium* has only two cirri, yet each of theſe being divided in two, they appear as four; ſo that WILLOUGHBY might juſtly ſay, that it is quadri-cirratus. The ſame author places the *Ophidium* in a genus which he calls

Enchelyopus, which is indeed not a good family, since it comprehends the genera of *Gymnotus*, *Anarrhichas*, *Cepola*, *Blennius*, *Cobitis*, &c.

LINNE in his description of the *Ophidium barbatum* says, that its whole body is covered with oblong spots, without any regular direction. Dr. GOUAN, in his description of the genus of the *Ophidium*, does not mention the scales; but gives the spots as a generic character. The last author who has mentioned these spots, and given a description of this fish, is Mr. BRUNNICHE in his *Ichthyologia Massiliensis*.

Having adduced the various opinions of the writers on the *Ophidium*, and endeavoured to reconcile their sentiments, we now proceed to give the description of this fish, which is so very remarkable for its singularities. The genus of *Ophidium* has the following principal characters, *viz.* the body long; the fins of the back, tail, and anus, confounded in one; no fin on the under part of the body; and the eyes covered by the common skin. There are besides many other characters which it is needless to observe here, since I intend not to describe all the species of the genus, but only to mention them. The first species (which is the species of which we are treating) is distinguished by its cirri. The second differs from the former not only by the absence of the cirri, but also by many other marks. ARTEDI, in his account of this species, has adopted the synonymy of SCHONEVELDE, who describes a fish under the name of *Ophidion imberbe flavum*; but this fish, which is the *Blennius gunnellus* LINN. is certainly very different from the *Ophidion imberbe*, LINN. the rays of its dorsal fin being prickly; which circumstance perhaps induced LINNE to place the *Ophidium* among the *Acanthopterygii* in the first editions of his Systema, in which he followed the classification of ARTEDI. Perhaps, for the same

ſame reaſon, that author has placed among the *Jugulares* in his *Fauna Suecica*, the ſpecies of which we are now ſpeaking. This fiſh is alſo very different from the *Sea ſnail* deſcribed by WILLOUGHBY and PETIVER, which is the *Cyclopterus lipparis* LINN. though the ſynonymy of PETIVER has been accepted by GRONOVIUS as being the *Ophidium imberbe* *.

LINNE, in the tenth edition of his Syſtema, mentions a third ſpecies of *Ophidium*, which he calls *Macrophthalmum*; but afterwards diſcovered that this ſpecies belonged to the genus of *Cepola*. It is another ſpecies of this genus which OTHO FABRICIUS has lately deſcribed in his *Fauna Græenlandica*, under the denomination of *Ophidium viride*. There are two other fiſhes, of which GRONOVIUS made a new genus, aſſigning to it the name of *Maſtacembelus*, which comes near to the genus of *Ophidium*, though I am perſuaded it conſtitutes a different one by the preſence of the aculei on the back. I ſhall now proceed to give a deſcription of that ſpecies of which I am particularly treating, which I have written in Latin for the ſake of perſpicuity.

DESCRIPTIO OPHIDII BARBATI LINN.

B. 7. D. 124. P. 20. V. 0. A. 115. C. — —.

CAPUT compreſſum, ſub-acutum, nudum, cute communi laxe tectum.

Rictus amplus.

Mandibula ſuperior duplicata, inferiore paulo longior.

* The fiſh mentioned by Mr. PENNANT, in the Britiſh Zoology, tom. III. append. p. 346. and engraved in the tab. 93. tom. II. under the name of *Ophidium imberbe* LINN. is a ſpecies of *Muræna*.

Labia cutacea, tenuia.

Dentes in utriuſque maxillæ margine, in aream anguſtam antice latiuſculam diſpoſiti, minuti, acuti, conferti, anteriores paululum majores.

Lingua ſubobtuſa, glabra.

Palatum medio glabrum, antice areis tribus denticulorum exaſperatum, quarum duæ laterales lineares, intermedia ſubtriangularis.

Fauces ſuperne exaſperatæ duabus areis longitudinalibus denticulorum.

Narium foramina utrinque bina, ante oculos poſita, diſtantia; foramine anteriore minore, poſteriore nudo.

Oculi ſuprèmi, vicini, magni, cute communi tecti: *iris* argentea; *pupilla* luteſcens.

Operculo branchiarum cute communi tecta, rotundata polyphylla, mollia.

Membrana branchioſtega, cute communi tecta, radiis ſubæqualibus.

Branchiæ quaternæ; tres interiores parte concava gerunt tubercula aſpera, ſeparata, in duas ſeries diſpoſita: quarta cæteris longior, latere interno gerit tubercula aſpera, externo radios uno latere læviter ſerratos.

Cirri duo qd apicem maxillæ inferioris, bipartiti; *lacinia* altera longiore.

Tuberculum ſubacutum, ſeu apophyſis oſſea tecta, recumbens, ante oculos in fronte poſita.

CORPUS. Compreſſum, verſus caudam attenuatum.

Linea lateralis ſuprema, lævis, dorſo parallela, vitta argentea ſubtus ornata.

Squamæ obovatæ, tectæ, umbonatæ, ſeparatæ.

Pinna

Pinna dorſalis longa * anali longior ſed anguſtior, cum caudali continuata, baſi ſordide albeſcens, margine nigra e punctis plurimis nigris: *Radii* ſimplices, flexiles, molles.

Pinnæ pectorales obovatæ, pellucidæ, membrana punctis minutiſſimis irrorata.

Pinna analis caudali unita, baſi albeſcens, margine nigra, radiis ſimplicibus.

Pinna caudalis nigra, apice obtuſa.

Color capitis et corporis carneo-argenteus.

Lc. : A :: 85 : 35 Lc. : Pl :: 85 : 15
Lc. : Dl :: 85 : 27 Lc. : Pf :: 85 : 22
Lc. : DFCFAF :: 85 : 87 Lc. : Al :: 85 : 37

R. 5/7. P. 2. D. 2. 3. 4. 5. 6. A. 3. 4. 5. 6.

Before I proceed, I think it neceſſary to explain the proportional meaſures which I employed in the foregoing deſcription. ARTEDI conſidered the meaſurement of fiſhes as neceſſary to render the deſcription of them perfect; and therefore in the ſpecies which he deſcribed moſt completely, we find, he did not fail to expreſs, with the greateſt accuracy, the diſtances between the ſeveral parts. He has been followed in this by the beſt authors who have written upon fiſhes, as GRONOVIUS and PALLAS. The like practice has been followed by ſome writers in deſcribing animals of other claſſes; and particularly by Mr. DAUBENTON, who has, with extraordinary exactneſs, expreſſed the meaſurements of quadrupeds. The uſe of this meaſurement, however, can only aſcertain that ſuch or ſuch individual is larger or leſs than another deſcribed by an author: for it is impoſſible by ſuch means to determine a ſpecies, and therefore a ſingle meaſurement, taken from the head to the tail, or in

* Pinna mihi audit longa, cujus baſis longitudine excedit radium altiorem.

ſhort

ſhort from any one part to another, would anſwer the ſame purpoſe; but if you take the terms of the meaſures, and compare them one with another, or all of them to a ſingle one, a method may be deduced for determining the ſpecies, and deſcribing each with accuracy, and that is the method which I uſed.

There is no doubt that fiſhes have a regular growth, and that all their parts have a proportional increaſe: it is therefore of no conſequence whether the meaſure be taken from an old or a young ſubject, it will be always of the ſame uſe for determining the ſpecies. The term to which I refer all the reſt, is taken from the diſtance between the apex of the upper jaw, and the baſis of the fin of the tail. The extremity of the upper jaw is the point from which I take the diſtance to all the other parts. To determine with more accuracy the junction of the fin to the tail, I incline the fin ſo as to form an angle with the tail; I take the diſtances with a pair of compaſſes, to avoid the irregularities of the ſurface of the body, which are infinitely various. When I have taken the diſtance with the compaſſes, I make a reference to a rule, which is divided in Engliſh inches, each of them ſub-divided in tenths. To exemplify my practice in a fiſh of the ſuppoſed length of 40 lines from the upper jaw to the tail, and from the ſame point to the anus of 20 lines, I ſay, that the diſtance from the head to the tail compared with the diſtance from the head to the anus is in this ſpecies as 40 to 20, which I expreſs in this manner: Lc. : A :: 40 : 20, which I thus reduce, Lc. : A :: 10 : 5. I then take the length from the upper jaw to the beginning of the dorſal * fin, and to its end: next from

* I meaſure the dorſal and anal fins at their baſis, never in their margin.

the

the ſame point to the baſis of the pectoral, to its extremity; and in the like manner for the ventral, anal, and tail fins; all which meaſures I refer to the common term 40. Admitting then the dorſal fin is diſtant from the upper jaw as ten, I write as before LC : DI :: 40 : 10, which I reduce, if I chuſe ſo, Lc. : DI :: 20 : 5.

I take the meaſures in length only, and never in breadth or depth, becauſe ſuch meaſures, on many accounts, for inſtance, from the quantity of ſpawn, from that of food in the ventricle, &c. would be uncertain; beſides there are terms enough afforded by the ſituation of the anus and the fins. I pay no regard to the meaſure of the noſtrils, of the eyes, &c. as theſe parts lie too near the extremity of the upper jaw to admit of a ſufficient accuracy in their meaſure. All theſe diſtances are written in the ſhorteſt manner poſſible; I therefore do not go into the inquiry what claſs of the LINNÆAN ſyſtem the fiſh deſcribed may belong to; or whether the anal fin is before or behind the region of the dorſal; whether the pectoral are extended beyond the ventral; or the tail is forked or not; and many other particulars which could not be expreſſed but by a very long deſcription, tedious both to the writer and reader.

To make an application of the proportional meaſurements, I ſuppoſe a fiſh, of a ſpecies deſcribed, to be in length 40 lines, and from the upper jaw to the anus 10: I take a ſpecimen which I ſuſpect to be of the ſame ſpecies, but which I wiſh to determine with certainty: the length of it is 20 lines; then I ſay 40 is to 10 as 20 is to another term, which I diſcover by multiplying 20 by 10, and dividing the product by 40, which will produce 5, the number of the lines comprehended between the upper jaw and the anus, if the fiſh be of the ſpecies it

it is thought to be. I proceed in the like manner, in comparing the terms of the measures of the other parts; but the utility of these measures is very apparent in the distinguishing of the species of some genera which are so natural as that of *Cyprinus*, *Clupea*, and many others, of which the species could not be otherwise easily distinguished. I shall instance a species of *Perca* described in the *Commentarii Petropolitani* under the name of *Perca acerina*, by Mr. GUELDENSTAEDT, which could not have been distinguished from the *Perca cernua* but by the proportional measures; since the *Cernua* has the body about three times longer than the head, whereas the *Acerina* has it but twice as long, though by the other characters they are almost indistinguishable.

To express the position of the fins briefly, and with all possible accuracy, which, however, I think may be very well understood by the described measures, I take the distance from the upper jaw to the basis of the pectoral fin, and then see into how many equal parts the whole body may be divided, and to these parts I apply the name of regions; I measure them to the extremity of the middle of the fin of the tail, and I express the position of the fins as follow: D. 3. 4. A. 3. V. 2. P. 2. the letters being the initials of the fins, and the numbers of the regions, the first being from the head to the pectoral fin *.

The scales of the *Ophidium*, which have been figured by RONDELETIUS, but overlooked by many other writers, have

* I beg pardon for the digression; but I thought it would not be improper in this place to observe, that the utility of this method of measurement will appear not only in distinguishing fishes, but also animals of other classes, and particularly snakes, which cannot be well determined otherwise. Besides, I do not know any author on subjects of natural history, who has adopted that method.

been mentioned by WILLOUGHBY, but without any particular deſcription. They are very different from thoſe on the ſkin of the *Ophidium imberbe*, which are ſhortly deſcribed by GRONOVIUS. Their poſition, as may be ſeen in the figure, is irregular. They are diſperſed over the whole body. Their form is ſometimes round, ſometimes nearly oval. They are largeſt near the head, and in the lower part of the body; but are hardly to be diſtinguiſhed near the tail. They adhere to the body by means of a particular tranſparent ſkin, which is in general very thin, but ſomewhat thicker near the neck, and extended looſely over the whole head: this ſkin is very eaſily deſtroyed, after which the ſcales falling, the body appears ſpotted (fig 1.). Theſe ſcales are of the ſame ſort as thoſe that LEEUWENHOEK has deſcribed on the eel, like thoſe I have ſeen on the *Anarrhichas lupus*, the *Blennius viviparus*, and many other fiſhes, which are commonly thought to be without ſcales. When you look at them with the naked eye (fig. 2.) they appear as covered with very ſmall grains; but viewed through a microſcope (fig. 3.) the middle of them appears more elevated than the margin; and from the center to the margin, cloſe by each other, there are many lines or rays, formed by ſmall ſcales placed one upon another, like tiles upon a roof, the ſuperior being always the nearer to the center. This ſort of ſcales, which may be called *umbonatæ*, are faſtened to the body by very ſmall veſſels which are inſerted in their middle; they are to be ſeen on the body only, not on the head nor the fins.

I ſhall now proceed to the anatomy of this fiſh, which certainly comprehends ſome very remarkable circumſtances, which, I believe, have not yet been obſerved in any other ſpecies. When we have drawn off the ſkin there appears a thin membrane of a ſilver colour, which covers the muſcles. The

muscles being removed, we find the peritoneum, which lines the abdominal cavity, and is adherent to the swimming bladder by some elongations. It is of a silver hue, with some very small black points. The ventricle is not to be distinguished from the intestines by any other mark but by its size: its form is oblong; it is extended almost to the anus, from whence the intestinal duct has a retrograde course, and then descends again, having a little dilatation near the anus. On the vertebræ next the anus on the outside of the peritoneum is a kind of cavity of an oblong form, containing a reddish viscus, which I take to be the kidney.

The first vertebra from the head has nothing very remarkable in its structure. The second has on each side an elongated and sharp apophysis, to the apex of which is annexed a small ligament. The third is very flat, and has on each side a kind of triangular and sharp apophysis, to which adheres a ligament as to the second. The fourth is remarkable in having a sharp apophysis on each side, articulated with the body of the vertebra, and under each of them, is another articulated apophysis, flattish, thick, roundish at its extremities, and forked at its basis (fig. 5.). The fifth, which is strongly adherent to the former, has on its middle a bifid process. The sixth has in its middle a flattish elevation, sharp on each side. Between the extremity of the larger apophysis of the fourth vertebra is a bone, or rather a hard cartilage, which bears the figure of a kidney (fig. 6.) its convexity being turned towards the body of the vertebra: its position is parallel to the bodies of the vertebræ; its motion is half circular; one of its parts, viz. the lowest, being in the cavity of the swimming bladder, to which it adheres by a thin membrane, so that no air can escape at that part. It is covered by membranes, which adhere strongly to its middle, in this part are fastened the two liga-

ments

ments of the apophyſis of the ſecond and third vertebræ, of which we ſpoke before, and which are of a great tenuity. In the ſame point are faſtened alſo two ligaments each of which belongs to an oblong muſcle parallel to each other, and fixed to the bones of the loweſt and poſterior part of the head (fig. 4.).

All this apparatus is certainly ſubſervient to the purpoſe of ſwimming, I ſuppoſe, by the cavity of the bladder being made larger or leſs by the motions of the cartilagineous bone; but it is very remarkable, that if theſe parts are neceſſary to ſome animal function, they ſhould not be found in all the individuals; for I have ſeen two, of which the vertebræ were not different from the vertebræ of the other ſpecies: which difference depends, perhaps, on the difference of ſex. I am inclined to believe ſo; but the generation in this fiſh ſeems to be no leſs myſterious than that of the eel: I could never diſtinguiſh a male from a female in this ſpecies. I do not know if the other ſpecies of *Ophidium* have the ſame ſtructure; I could not perceive it in ſome ſpecimens of *Maſtacembelus*. WILLOUGHBY mentions that ſingular ſtructure, but without any particular deſcription.

This fiſh commonly grows to the ſize of eight or nine inches. It is to be found in all the Mediterranean Sea, and in great plenty in the Adriatic. It is taken by nets in Provence and Languedoc, together with many other ſmall ſpecies, which are not eſteemed, that is, what they call *Ravaillā*. It is often confounded with the *Cepola* by the fiſhermen, though they have different names for each ſpecies. In Languedoc the *Ophidium* is called *Donzellā*, and the *Cepola*, *flammā*. In Provence the former has the name of *Corrudgiaō*, and the latter that of *Rougeollā*. But the name of *Donzellā*, very common on all the coaſt of the Mediterranean, is alſo applied to the *Cepola*, and the *Sparus julis*

LINN. which, however, is commonly called *girellā*. In ſummer the *ophidium* is more common: its fleſh is not of a good taſte, rather coarſe, as that of all the ſpecies of fiſhes, which having no ventral fins, are obliged to make great efforts in ſwimming, and have conſequently the muſcles harder. The want of ventral fins induces me to believe, that it is not a migratory ſpecies. It feeds upon ſmall crabs and fiſhes.

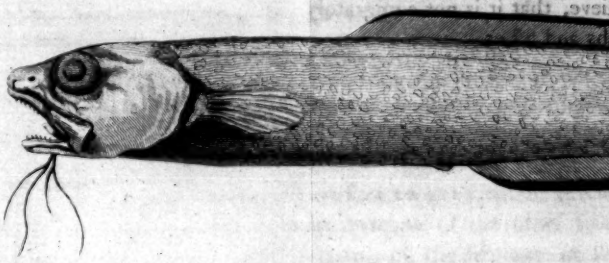
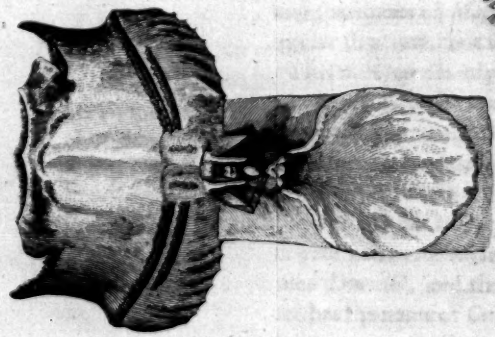


Fig. 1.

Fig 4.



Ophid

Fig. 5.

Fig. 6.

Fig. 3.

Fig. 2.

ophidium barbatum Linn.

XXIX. *A further Account of the Uſefulneſs of waſhing the Stems of Trees.* By Mr. Robert Marſham, *of* Stratton, *F. R. S.*

Read May 31, 1781.

THE following account is a kind of poſtſcript to my letter to Dr. Moss, Lord Biſhop of Bath and Wells, in 1775, which the Royal Society did me the honour to publiſh in the Philoſophical Tranſactions in 1777. In that I ſhewed how much a Beech increaſed upon its ſtem being cleaned and waſhed *; and in this I ſhall ſhew, that the benefit of cleaning the ſtem continues ſeveral years: for the Beech which I waſhed in 1775 has increaſed in the five years ſince the waſhing eight inches and ſix-tenths, or above an inch and ſeven-tenths yearly; and the aggregate of nine unwaſhed Beeches of the ſame age does not amount to one inch and three-tenths yearly to each tree. In 1776 I waſhed another Beech (of the ſame age, *viz.* ſeed in 1741); and the increaſe in four years ſince the waſhing is nine inches and two-tenths, or two inches and three-tenths yearly, when the aggregate of nine unwaſhed Beeches amounted to but one inch and three-tenths and a half. In 1776 I waſhed an Oak which I planted in 1720, which has increaſed in the four years ſince waſhing ſeven inches and two-tenths, and the

* Vide Phil. Tranſ. vol. LXVII. for the year 1777, part I. p. 12.

aggregate of three Oaks planted the ſame year (*viz.* all I meaſured) amounted to but one inch yearly to each tree. In 1779 I waſhed another Beech of the ſame age, and the increaſe in 1780 was three inches, when the aggregate of fifteen unwaſhed Beeches was not full fifteen inches and ſix-tenths, or not one inch and half a tenth to each tree; yet moſt of theſe trees grew on better land than that which was waſhed. But I apprehend the whole of the extraordinary increaſe in the two laſt experiments ſhould not be attributed to waſhing: for in the autumn of 1778 I had greaſy pond-mud ſpread round ſome favourite trees, as far as I ſuppoſed their roots extended, and although ſome trees did not ſhow to have received any benefit from the mud, yet others did, that is, an Oak increaſed half an inch, and a Beech three-tenths, above their ordinary growth. Now though the Beech gained but three-tenths, yet, perhaps, that may not be enough to allow for the mud; for the ſummer of 1779 was the moſt ungenial to the growth of trees of any ſince I have meaſured them, ſome not gaining half their ordinary growth, and the aggregate increaſe of all the unwaſhed and unmudded trees that I meaſured (ninety-three in number of various kinds) was in 1779 but ſix feet five inches and ſeven tenths, or ſeventy-ſeven inches and ſeven-tenths, which gives but eight-tenths and about one-third to each tree; when in 1778 (a very dry ſummer in Norfolk) they increaſed ſeven feet and nine-tenths, or near eighty-five inches, which gives above nine-tenths to each tree: and this ſummer of 1780 being alſo very dry, yet the aggregate increaſe was above half an inch more than in 1778. But the beſt increaſe of theſe three years is low, as there are but twenty of the ninety-three trees that were not planted by me, and greater increaſe is reaſonably expected in young than old trees;

yet

yet I have an Oak now two hundred years old* (1780) which is ſixteen feet and five inches in circumference, or one hundred and ninety-ſeven inches in two hundred years. But this Oak cannot properly be called old. The annual increaſe of very old trees is hardly meaſurable with a ſtring, as the ſlighteſt change of the air will affect the ſtring more than a year's growth. The largeſt trees that I have meaſured are ſo far from me, that I have had no opportunity of meaſuring them a ſecond time, except the Oak near the honourable Mr. LEGGE's Lodge in Holt Foreſt, which does not ſhow to be hollow. In 1759 I found it was at ſeven feet (for a large ſwelling rendered it unfair to meaſure at five or ſix feet) a trifle above thirty-four feet in circumference, and in 1778 I found it had not increaſed above half an inch in nineteen years. This more entire remain of longevity merits ſome regard from the lovers of trees, as well as the hollow Oak at Cawthorp in Yorkſhire, which Dr. HUNTER gives an account of in his edition of EVELYN's Silva, and calls it forty-eight feet round at three feet. I did not meaſure it ſo low; but in 1768 I found it at four feet, forty feet and ſix inches; and at five feet, thirty-ſix feet and ſix inches; and at ſix feet, thirty-two feet and one inch. Now, although this Oak is larger near the earth than that in Hampſhire, yet it diminiſhes much more ſuddenly in girt, *viz.* eight feet and five inches in two feet of height (I reckon by my own meaſures as I took pains to be exact). Suppoſe the diminution continues about this rate (for I did not meaſure ſo high) then at ſeven feet it will be about twenty-eight feet in circumference, and the bottom

* I cannot miſtake in the age of this Oak, as I have the deed between my anceſtor ROBERT MARSHAM and the Copyhold Tenants of his Manor of Stratton, dated May 20, 1580, upon his there incloſing ſome of his waſte; and the abuttal is elm.

fourteen feet contain ſix hundred and eighty-ſix feet round or buyers meaſure, or ſeventeen ton and ſix feet; and fourteen feet length of the Hampſhire Oak is one thouſand and ſeven feet, or twenty-five ton and ſeven feet, that is, three hundred and twenty-one feet more than the Yorkſhire Oak, though that is ſuppoſed by many people the greateſt Oak in England.

I am unwilling to conclude this account of waſhing the ſtems of trees without obſerving, that all the ingredients of vegetation united, which are received from the roots, ſtem, branches, and leaves of a moſſy and dirty tree, do not produce half the increaſe that another gains whoſe ſtem is clean to the head only, and that not ten feet in height. Is it not clear that this greater ſhare of nouriſhment cannot come from rain? for the dirty ſtem will retain the moiſture longer than when clean, and the nouriſhment drawn from the roots, and imbibed by the branches and leaves, muſt be the ſame to both trees. Then muſt not the great ſhare of vegetative ingredients be conveyed in dew? May not the moſs and dirt abſorb the fineſt parts of the dew? and may they not act as a kind of ſcreen, and deprive the tree of that ſhare of air and ſun which it requires? To develope this myſterious operation of nature would be an honor to the moſt ingenious, and the plain fact may afford pleaſure to the owners of young trees; for if their growth may be increaſed by cleaning their ſtems once in five or ſix years (and perhaps they will not require it ſo often) if the increaſe is but half an inch yearly above the ordinary growth, it will greatly over-pay for the trouble, beſides the pleaſure of ſeeing the tree more flouriſhing. Although the extra increaſe of my firſt waſhed Beech was but four-tenths of an inch, the ſecond was nine-tenths and a half, and the third near two inches, ſo the aggregate extra increaſe is above one inch and

one-

one tenth yearly; and the increaſe of the oak is eight-tenths. But calling it only half an inch, then ſix years will produce above five cubic feet of timber, as the oak is eight feet round, and above twenty feet long, and ſix pence will pay for the waſhing, ſo there remains nine ſhillings and ſix pence clear gain in ſix years.

Stratton,
Oct. 29, 1780.

XXX. *Hints relating to the Uſe which may be made of the Tables of natural and logarithmic Sines, Tangents, &c. in the numerical Reſolution of adfected Equations.* By William Wales, *F. R. S. and Maſter of the Royal Mathematical School in* Chriſt's Hoſpital.

Read June 14, 1781.

THE firſt intimation that I can meet with relating to the uſe which may be made of the tables of ſines, tangents, and ſecants, in reſolving adfected equations, is in the latter part of the ſecond volume of Profeſſor SAUNDERSON's Elements of Algebra, printed in 1741, after his deceaſe. The profeſſor there ſhews how to reſolve thoſe two caſes which make the firſt and ſecond of the following examples, by means of the tables; but it appears, from many circumſtances, he was not aware that the third caſe could be reſolved in the ſame manner. All the three forms were, however, reſolved by the late Mr. ANTHONY THACKER, a very ingenious man, who died in the beginning of the year 1744, by the help of a ſet of tables, of his own invention; different from, but in ſome meaſure analogous to, the tables of ſines and tangents. Theſe tables were finiſhed and publiſhed, together with ſeveral papers concerning them, after his death, by a Mr. BROWN of Cleobury. In theſe papers, beſide explaining fully the uſe of the tables in reſolving cubic equations, Mr. THACKER ſhews that his method comprehends the reſolution of all biquadratic equations, if they be

firſt reduced to cubic ones in the manner which has been explained by DESCARTES and others, and the ſecond term then taken away.

Since that time M. MAUDUIT has ſhewn how to find the roots of all the three forms of cubic equations, by means of the tables of ſines, &c. in his excellent Treatiſe of Trigonometry. But none of theſe authors have attempted to reſolve equations of more dimenſions than three, by theſe means, without firſt reducing them to that number; nor even theſe, before the ſecond term, or that which involves the ſquare of the unknown quantity, is taken away: whereas ſuch reductions will generally take up more time than is required to bring out the value of the unknown quantity by the following method; and, after all, frequently ſerve no other purpoſe but that of rendering the operation more intricate and troubleſome.

The truly ingenious Mr. LANDEN, in his lucubrations, publiſhed in 1755, has given a general method of reſolving that caſe of cubic equations, by means of the tables of ſines, where all the roots are real, without the trouble of taking away the ſecond term of the equation: and Mr. SIMPSON has ſhewn how to reſolve equations of any dimenſions, by the ſame means, provided thoſe equations involve only the odd powers of the unknown quantity, and that the co-efficients obſerve ſuch a law as will reſtrain the equation to that form which is expreſſive of the coſine of the multiple of an arc, of which the unknown quantity is the coſine, This was firſt done, I believe, by JOHN BERNOULLI, and afterwards by Mr. EULER, in his *Introduct. ad Analyt. Infinit.* and Mr. DE MOIVRE, in his *Miſcell. Analyt.*; but the reſolution of all equations of this form, as well as many others, is comprehended in the firſt of the following obſervations.

The firſt thought of extending the uſe of the tables of ſines, tangents, and ſecants, farther than to the caſes which have been already mentioned, occurred to me while I was conſidering the problem which produced the equation given in this paper as the fourth example. And it is remarkable, that the very ſame thought occurred to Dr. HUTTON about the ſame time, and in the reſolution of the ſame problem; and we were not a little ſurprized, on comparing our ſolutions together, to find that our ideas had taken ſo exactly the ſame turn; and that both ſhould have ſtumbled on a thought, which, as far as either of us knew, had never preſented itſelf to any one before. Having ſince examined farther into the matter, I have the ſatisfaction to find, that the principle is very extenſive, and that a great number of equations, eſpecially ſuch as ariſe in the practice of geometry, aſtronomy, and optics, may be reſolved by it with great eaſe and expedition.

But beſide the facility with which the value of the unknown quantity is brought out by means of the tables of ſines, tangents, and ſecants, this method of reſolution has another conſiderable advantage over moſt others which have been propoſed, inaſmuch as the firſt ſtate of the equation, without any previous reduction, is generally the beſt it can be in for reſolution; and from which it may moſt readily be diſcovered, how to ſeparate it into ſuch parts as expreſs the ſine, or the tangent, or the ſecant of the arc of a circle; or into the ſine, tangent, or ſecant of ſome multiple of that arc, or of a part of it; and in the doing of which conſiſts the principal part of the buſineſs in queſtion. It will alſo be of ſome advantage to preſerve the original ſubſtitutions as diſtinct as poſſible, by uſing only the ſigns of the ſeveral operations which it may be neceſſary to go through

through in bringing the ſolution of a problem to an equation, inſtead of performing the operations themſelves.

Beſides the advantages which this method of procedure affords to the mode of reſolution now more particularly under conſideration, it has ſo many others over that which is commonly made uſe of, that I am much ſurprized the latter ſhould ever have been adopted. By preſerving thus the original ſubſtitutions diſtinct, all the way through an operation, every expreſſion, even to the final equation, will exhibit the whole proceſs up to that ſtep; and it will appear as clearly, how every expreſſion has been derived, as it does in that mode of analyſis which was uſed by the ancient geometricians: whereas, when the ſeveral original expreſſions are melted down into one maſs by the multitude of actual additions, ſubtractions, multiplications, and diviſions, which they generally undergo, in a long algebraical proceſs, conducted in the uſual manner, it is impoſſible to trace the ſmalleſt veſtige of the original quantities in the final equation, except ſuch as are repreſented by a ſingle letter. Of courſe, however obvious the ſeveral ſteps might be at the time when they were taken, every idea of them muſt be totally loſt in the reſult; and it will be utterly impoſſible to trace them back again, in the manner they are done in the compoſition of a problem, the ſolution of which has been inveſtigated by the geometrical analyſis*. Let me add, that it is to this cauſe

* This ſubject, if ever I am bleſſed with more leiſure than is at preſent my lot, ſhall be purſued farther in another paper: in which I ſhall endeavour to ſhew, that, notwithſtanding the great difference which there appears to be between algebra and geometry, they are really but one ſcience, differently treated; and that the operations of the former may be rendered as clear and perſpicuous as thoſe of the latter are allowed to be. A diſquiſition of this nature will at leaſt have the merit of reſcuing a very uſeful and expeditious mode of inveſtigation from

cauſe we muſt attribute all that obſcurity which the algebraic mode of inveſtigation has been ſo frequently charged with.

I ſhall endeavour to verify this doctrine, in ſome meaſure, by the expreſſions which are put down in the following tables for the ſines, coſines, and tangents of arcs of circles, and of the multiples of thoſe arcs; which tables will be found very uſeful in the proſecution of the deſign which I am now upon, and are abſolutely neceſſary in the explanation of it.

from an unmerited ſtigma: and if I never be happy enough to have an opportunity of doing it myſelf, what I have here ſaid may be the means of putting ſome other perſon, who has, upon it.

TABLE

T A B

Arc.	Sine.	Cosine.
A	x	$\sqrt{r+x \cdot r$
2A	$\frac{2x}{r} \cdot \sqrt{r+x \cdot r-x}$	$\frac{\overline{r+x} \cdot \overline{r-x}}{x}$
3A	$\frac{3x \cdot \overline{r+x} \cdot \overline{r-x} - x^3}{r^2}$	$\frac{\overline{r+x} \cdot \overline{r-x} - 3x^2}{r^2} \sqrt{$
4A	$\frac{4x \cdot \overline{r+x} \cdot \overline{r-x} - 4x^3}{r^3} \sqrt{r+x \cdot r-x}$	$\frac{\overline{r+x}^2 \cdot \overline{r-x}^2 - 6x^2 \cdot}{r^3}$
5A	$\frac{5x \cdot \overline{r+x}^2 \cdot \overline{r-x}^2 - 10x^3 \cdot \overline{r+x} \cdot \overline{r-x} + x^5}{r^4}$	$\frac{\overline{r+x}^2 \cdot \overline{r-x}^2 - 10x^2 \cdot \overline{r+x} \cdot}{r^4}$
6A	$\frac{6x \cdot \overline{r+x}^2 \cdot \overline{r-x}^2 - 20x^3 \cdot \overline{r+x} \cdot \overline{r-x} + 6x^5}{r^5} \sqrt{r+x \cdot r-x}$	$\frac{\overline{r+x}^3 \cdot \overline{r-x}^2 - 15x^2 \cdot \overline{r+x}^2 \cdot}{r^5}$

T A B

Arc.	Sine.	Cosine.
A	$\frac{rx}{\sqrt{r^2+x^2}}$	$r \cdot \frac{r}{\sqrt{r^2+x}}$
2A	$\frac{2r^3x}{r^4+x^4}$	$r \cdot \frac{\overline{r+x} \cdot}{r^3+x}$
3A	$\frac{\overline{r+x} \cdot \overline{r-x} + 2r^2}{r^3+x^3} \times \frac{rx}{\sqrt{r^2+x^2}}$	$r \cdot \frac{\overline{r+x} \cdot \overline{r-x} - 2r}{r^3+x^3}$
4A	$\frac{2 \cdot \overline{r+x} \cdot \overline{r-x}}{r^2+x^2} \times \frac{2r^2x}{r^2+x^2}$	$r \cdot \frac{\overline{r+x} \cdot \overline{r-x} - 2rx}{r^2+x^2} \times$
5A	$\frac{\overline{r+x}^2 \cdot \overline{r-x}^2 + 4x^2 \cdot \overline{r+x} \cdot \overline{r-x} - 4r^2x^2}{r^2+x^2 \cdot r^2+x^2} \times \frac{rx}{\sqrt{r^2+x^2}}$	$r \cdot \frac{\overline{r+x}^2 \cdot \overline{r-x}^2 - 4x^2 \cdot \overline{r+x} \cdot}{r^2+x^2 \cdot r^2+x^2}$
6A	$\frac{3 \cdot \overline{r+x}^2 \cdot \overline{r-x}^2 - 4x^2x^2}{r^2+x^2 \cdot r^2+x^2} \times \frac{2 \cdot r^2x}{r^2+x^2}$	$\frac{\overline{r+x}^3 \cdot \overline{r-x}^2 - 12r^2x^2}{r^6+x^6} \times$

TABLE I.

Cofine.	Tangent.
$\sqrt{r+x \cdot r-x}$	$\frac{rx}{\sqrt{r+x \cdot r-x}}$
$\frac{r-x-x^2}{x}$	$\frac{2}{r+x \cdot r-x-x^2} \sqrt{r+x \cdot r-x}$
$\frac{3x^2}{\sqrt{r+x \cdot r-x}}$	$\frac{3 \cdot r+x \cdot r-x-x^2}{r+x \cdot r-x-3x^2} \times \frac{rx}{\sqrt{r+x \cdot r-x}}$
$\frac{6x^3 \cdot r+x \cdot r-x+x^4}{r^3}$	$\frac{4 \cdot r+x \cdot r-x-4x^2}{r+x^2 \cdot r-x^2-6x^2 \cdot r+x \cdot r-x+x^4} \sqrt{r+x \cdot r-x}$
$\frac{-x \cdot r-x+5x^4}{\sqrt{r+x \cdot r-x}}$	$\frac{5 \cdot r+x^2 \cdot r-x^2-10x^2 \cdot r+x \cdot r-x+x^4}{r+x^2 \cdot r-x^2-10x^2 \cdot r+x \cdot r-x+5x^4} \times \frac{rx}{\sqrt{r+x \cdot r-x}}$
$\frac{\cdot r-x^2+15x^4 \cdot r+x \cdot r-x-x^6}{r^3}$	$\frac{6 \cdot r+x^2 \cdot r-x^2-20x^2 \cdot r+x \cdot r-x+6x^4}{r+x^3 \cdot r-x^3-15x^2 \cdot r+x^2 \cdot r-x^2+15x^4 \cdot r+x \cdot r-x} \cdot \sqrt{r+x \cdot r-x}$

TABLE II.

Cofine.	Tangent.
$\frac{r}{\sqrt{r^2+x^2}}$	x
$\sqrt{r+x \cdot r-x}$	$\frac{2r^2x}{r+x \cdot r-x}$
$\frac{r^2+x^2}{x}$	$\frac{x \cdot r+x \cdot r-x+2r^2x}{r+x \cdot r-x-2x^2}$
$\frac{x-2rx}{r^2+x^2} \times \frac{r}{\sqrt{r^2+x^2}}$	$\frac{4r^2x \cdot r+x \cdot r-x}{r+x^2 \cdot r-x^2-4r^2x^2}$
$\frac{r+x \cdot r-x+2rx}{r^2+x^2} \times \frac{r}{\sqrt{r^2+x^2}}$	$\frac{x \cdot r+x^2 \cdot r-x^2+4r^2x \cdot r+x \cdot r-x-4r^2x^2}{r+x^2 \cdot r-x^2-4r^2x^2}$
$\frac{r+x \cdot r-x-4r^2x^2}{r^2+x^2} \times \frac{r}{\sqrt{r^2+x^2}}$	$r^2 \cdot \frac{6x \cdot r+x^2 \cdot r-x^2-8r^2x^2}{r+x^2 \cdot r-x^2-12r^2x^2 \cdot r+x \cdot r-x}$
$\frac{r+x \cdot r-x}{r^2+x^2} \times \frac{r}{r^2+x^2}$	

TABL

Arc.	Sine.	Cosine.
A	$\frac{r \cdot \sqrt{x+r \cdot x-r}}{x}$	$r \cdot \frac{r}{x}$
2A	$r \cdot \frac{2r^2 \cdot \sqrt{x+r \cdot x-r}}{x^2}$	$r \cdot \frac{r^2-x+r \cdot x}{x^2}$
3A	$r \cdot \frac{3r^3-x+r \cdot x-r}{x^3} \sqrt{x+r \cdot x-r}$	$r \cdot \frac{r^3-3r \cdot x+r \cdot x}{x^3}$
4A	$\frac{r \cdot 4r^3-4r \cdot x+r \cdot x-r}{x^3} \sqrt{x+r \cdot x-r}$	$r \cdot \frac{r^4-6r^2 \cdot x+r \cdot x-r+x}{x^4}$
5A	$r \cdot \frac{5r^4-20r^2 \cdot x+r \cdot x-r+x+r^2 \cdot x-r^2}{x^3} \sqrt{x+r \cdot x-r}$	$r \cdot \frac{r^5-10r^3 \cdot x+r \cdot x-r+5r}{x^5}$
6A	$r \cdot \frac{6r^5-20r^3 \cdot r+x \cdot r-x+6r \cdot x+r^2 \cdot x-r^2}{x^5} \sqrt{x+r \cdot x-r}$	$r \cdot \frac{r^6-15r^4 \cdot x+r \cdot x-r+15r^2 \cdot x+r^2}{x^6}$

TABL

Arc.	Sine.	Cosine.
A	$\sqrt{r^2-r-x^2}$	$r-x$
2A	$2 \cdot \frac{r-x}{r} \sqrt{r^2-r-x^2}$	$2 \cdot \frac{r-x^2-r^2}{r}$
3A	$\frac{4 \cdot r-x^2-r^2}{r^2} \sqrt{r^2-r-x^2}$	$\frac{4 \cdot r-x^3-3r^2 \cdot r-x}{r^2}$
4A	$\frac{8 \cdot r-x^3-4r^2 \cdot r-x}{r^3} \sqrt{r^2-r-x^2}$	$\frac{8 \cdot r-x^4-8r^2 \cdot r-x^2}{r^3}$
5A	$\frac{16 \cdot r-x^4-12r^2 \cdot r-x^2+r^4}{r^4} \sqrt{r^2-r-x^2}$	$\frac{16 \cdot r-x^5-20r^2 \cdot r-x^3+}{r^4}$
6A	$\frac{32 \cdot r-x^5-32r^2 \cdot r-x^3+6r^4 \cdot r-x}{r^5} \sqrt{r^2-r-x^2}$	$\frac{32 \cdot r-x^6-48r^2 \cdot r-x^4+18}{r^5}$

TABLE III.

&c.	Tangent.
	$\sqrt{x+r \cdot x-r}$
	$\frac{2r^2 \cdot \sqrt{x+r \cdot x-r}}{r^2-x+r \cdot x-r}$
	$\frac{3r^2-x+r \cdot x-r}{r-3 \cdot x+r \cdot x-r} \sqrt{x+r \cdot x-r}$
	$\frac{4r^4-4r^2 \cdot x+r \cdot x-r}{r^4-6r^2 \cdot x+r \cdot x-r+x+r^2 \cdot x-r^2} \sqrt{x+r \cdot x-r}$
	$\frac{5r^4-10r^2 \cdot x+r \cdot x-r+x+r^2 \cdot x-r^2}{r^4-10r^2 \cdot x+r \cdot x-r+5 \cdot x+r^2 \cdot x-r^2} \sqrt{x+r \cdot x-r}$
	$\frac{6r^6-20r^4 \cdot x+r \cdot x-r+6r^2 \cdot x+r^2 \cdot x-r^2}{r^6-15r^4 \cdot x+r \cdot x-r+15r^2 \cdot x+r^2 \cdot x-r^2-x+r^2 \cdot x-r^2} \sqrt{x+r \cdot x-r}$

TABLE IV.

&c.	Tangent.
	$\frac{r}{r-x} \sqrt{r^2-r-x^2}$
	$\frac{2r \cdot r-x}{2 \cdot r-z^2-r^2} \sqrt{r^2-r-z^2}$
	$\frac{4r \cdot r-z^2-r^2}{4 \cdot r-z^2-3r^2 \cdot r-x} \sqrt{r^2-r+x^2}$
	$\frac{8r \cdot r-x^3-4r^2 \cdot r-x}{8 \cdot r-x^4-8r^2 \cdot r-x^2+r^4} \sqrt{r^2-r-x^2}$
	$\frac{16r \cdot r-z^4-12r^2 \cdot r-x^2+r^4}{16 \cdot r-z^4-20r^2 \cdot r-x^2+5r^4 \cdot r-x} \sqrt{r^2-r-x^2}$
	$\frac{32r \cdot r-x^5-32r^3 \cdot r-x^3+6r^5 \cdot r-x}{32 \cdot r+x^6-48r^2 \cdot r-x^4+18r^4 \cdot r+x^2-r^6} \sqrt{r^2-r-x^2}$

Observations on the foregoing tables.

EACH of the formulæ in these tables may be considered as one side of an equation, involving the unknown quantity x to different dimensions. In some of the formulæ the odd powers of x are only found, in others the even ones alone, and in others both; but they are all equally useful in finding the value of the unknown quantity in adfected equations which contain all the powers of that quantity, as will plainly appear from the following considerations.

I. If, on bringing the solution of any problem to an equation with some known quantity, it be found to correspond with any of the formula in these tables; or, if by any means it can be reduced to any of them, it is manifest, that nothing remains to be done but to divide the known side of the equation by the value of the quantity which is here denoted by r, and to seek for the quotient in the tables of sines, cosines, or tangents, as the case may require, and the value of the unknown quantity will be the sine, tangent, secant, or versed sine, of a given part of that arc (according as the expression is found in the first, second, third, or fourth table) multiplied by the value of r.

II. If, as will more frequently happen, the final equation of an operation be found equivalent to the sum, difference, product, or quotient, of some two or more of these formula; or to the sum, difference, product, or quotient, of some two or more of them multiplied or divided, increased or lessened, by some known quantity or quantities; then, having taken away

the known quantities by the common algebraic rules, obſerve the following ones.

1ſt. When the equation is found to correſpond with the ſum or difference of two formula in theſe tables, which are the ſine and tangent, ſine and coſine, or coſine and tangent, of the ſame arc, by running the eye along the tables of natural ſines and tangents, find theſe two arcs, immediately following one another, the ſum or difference of the ſine and tangent, ſine and coſine, or coſine and tangent, of which are one of them greater, and the other leſs than the number which conſtitutes the known ſide of the equation. Take the exceſs of one of theſe ſums or differences above, and what the other ſum or difference wants of the ſaid given number, add theſe two errors together, and ſay, as the ſum of them is to 60″, ſo is that error which belongs to the leſs arc to a number of ſeconds; which being added to the leſs arc will give one, the ſum or difference of whoſe ſine and tangent, ſine and coſine, or coſine and tangent, is exactly equal to the number which conſtitutes the known ſide of the equation. Of the arc, thus found, let ſuch a part be taken as the table in which the formula are found directs, and the natural ſine, tangent, ſecant, or verſed ſine (as the caſe may require) of this part, being multiplied by the value of r, if r be found in the equation, will be the value of x ſought.

2d. When the equation happens to be the product or quotient of two formulæ which expreſs the ſine and coſine, ſine and tangent, or coſine and tangent, of the ſame arc, take the logarithm of the number which conſtitutes the known ſide of the equation, and then follow exactly the directions given in the firſt caſe, uſing the tables of logarithmic ſines and tangents inſtead of the tables of natural ones.

3d. If the equation, finally reſulting from the reſolution of any problem, preſent itſelf in an expreſſion which is compoſed of the ſum or difference of the ſine, coſine, or tangent, of an arc, of which the unknown quantity is the ſine, coſine, tangent, or verſed ſine, and the ſine, coſine, or tangent, of ſome multiple of that arc, it will then be convenient to have two tables of ſines and tangents; and in running the eye along them to find the two arcs immediately following one another, of which the ſum or difference of the ſine, coſine, or tangent, of one of them, and the ſine, coſine, or tangent, of ſome multiple of it, may be leſs, and the ſum or difference of the ſine, coſine, or tangent, of the other, and the ſine, coſine, or tangent, of the ſame multiple of it, may be greater than the number which conſtitutes the known ſide of the equation, for every minute of a degree that the finger is moved over in one, it muſt be moved over a number of minutes in the other, which is equal to the number of times that the ſingle arc is contained in the multiple one. When theſe two arcs are found, the operation will not differ ſo materially from that which is pointed out in the firſt rule as to merit repetition.

4th. If, inſtead of the ſum or difference of the ſine, coſine, or tangent, of an arc, and the ſine, coſine, or tangent, of ſome multiple of it, the form of the equation be ſuch as to be conſtituted of the product of them, or the quotient of one divided by the other, the laſt rule will ſtill hold good, uſing only the logarithmic ſines and tangents inſtead of the natural ones, and comparing the ſum or difference of them, according as the equation is compoſed of the product or quotient of the two factors, with the logarithm of the number which conſtitutes the known ſide of the equation, inſtead of that number itſelf.

5th. Sometimes the final equation will come out in expreſſions which are conſtituted of the ſum, difference, product, or quotient, of the ſine, coſine, or tangent, of ſome multiple of an arc, of which the unknown quantity is the ſine, tangent, ſecant, or verſed ſine, and the ſine, coſine, or tangent, of ſome other multiple of the ſame arc. And in any of theſe caſes it is manifeſt, that the method of proceeding, in order to obtain one of the multiple arcs, and from thence the ſingle one, of which the unknown quantity is the ſine, tangent, &c. will not be greatly different from thoſe which have been deſcribed in the third and fourth rules. The moſt material difference conſiſts in this, that inſtead of proceeding minute by minute, according to the directions in the third rule to find the ſingle arc, it will now be moſt convenient to proceed in each table by as many minutes at each ſtep as are equal in number to the number of times which the ſingle arc is contained in the multiple ones reſpectively.

6th. Equations will frequently make their appearance in formulæ which expreſs the ſquare, cube, &c. of the ſine, coſine, or tangent, of the multiple of ſome arc, of which the unknown quantity is the ſine, tangent, ſecant, or verſed ſine; or in formulæ which are expreſſive of the ſum, difference, product, &c. of the ſine, coſine, or tangent, of an arc, and ſome power of the ſine, coſine, or tangent, of the ſame arc; or of ſome multiple of it, the unknown quantity being ſome other trigonometrical line belonging to that arc. Or the equation may be compounded of the ſum, difference, product, &c. of the ſame, or different powers of the ſines, tangents, or coſines, of different multiples of an arc, the unknown quantity being the ſine, tangent, ſecant, or verſed ſine, of that arc. In every one of theſe caſes the tables will give the value of the unknown quantity,

quantity, and in moſt of them with great eaſe and expedition. The method which is to be purſued in each caſe will readily preſent itſelf to a ſkilful analyſt, who attends carefully to what has been already ſaid, and to the examples which follow.

IV. The formula in the four preceding tables may be greatly varied by ſuppoſing x, the unknown quantity, to be ſome part or parts of the ſine, tangent, &c. as $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{3}$, $\frac{1}{4}$, &c. or ſome multiple of it, as twice, thrice, &c. Or x may be the ſquare, or the ſquare root, or any other power of the ſine, tangent, ſecant, or verſed ſine, of an arc; in every one of which caſes the formula will put on different appearances, either with reſpect to the powers or co-efficients of the unknown quantity, and yet admit of the ſame kind of application.

V. The tables may be rendered yet more extenſively uſeful by inſerting expreſſions for the ſines, coſines, and tangents, of half the arc which has x for its ſine, tangent, ſecant, or verſed ſine; and alſo for the ſines, coſines, and tangents, of the odd multiples of this half arc, which expreſſions, together with thoſe already inſerted, may be conſidered as the ſines, coſines, and tangents, of the multiples of an arc, the unknown quantity, being the ſine, tangent, &c. of twice that arc. And this conſideration may ſometimes be applied to very uſeful purpoſes.

VI. In order to render the formula in the tables more general, I have put r for the radius of the circle; whereas it will frequently happen, that the equation, finally reſulting from the reſolution of a problem, eſpecially thoſe which relate to the doctrine of the ſphere, will preſent itſelf in a form where the radius muſt be taken equal to unity: what theſe forms are will readily appear by ſubſtituting unity for r and its powers every where in the expreſſion.

It would be endleſs were I to undertake to enumerate all the various circumſtances and caſes in which this method of bringing out the unknown quantity may be applied with ſucceſs: what has already been ſaid will be ſufficient to explain the nature of it, and to enable the analyſt to apply it in other inſtances as they occur to him, I ſhall therefore only add a few examples to illuſtrate it.

EXAMPLE I.

Let it be required to find the value of x in an equation of the form $x^3 - r^2x = a$.

If r^2 be expounded by 50, and a by 120 (ſee Phil. Tranſ. vol. LXVIII. p. 937.) the equation may be reduced to $\sqrt{x} \times \sqrt{x^2 - 50} = \sqrt{120}$; and, conſequently, by tab. III. if x be conſidered as the ſecant of an arc, of which the radius is $\sqrt{50}$, $\sqrt{x^2 - 50}$ will be the tangent of it, and we ſhall have to find an arc, ſuch that the tangent multiplied by the ſquare root of the ſecant may be equal to $\sqrt{120}$; or, which amounts to the ſame thing, ſuch an arc that the log. tang. together with half the log. ſecant may be equal to half the log. of 120. But becauſe the tangent and ſecant, here required, are to the radius of the $\sqrt{50}$, the log. tangents and ſecants in the tables muſt be increaſed by the logarithm of that number, and therefore log. tang. $+ \frac{1}{2}$ log. $50 + \frac{1}{2}$ log. ſecant $+ \frac{1}{2}$ log. $50 = \frac{1}{2}$ log. 120: or log. tang. $+ \frac{1}{2}$ log. ſecant $= \frac{1}{2}$ log. $120 - \frac{1}{2}$ log. of 50. Hence, having taken $\frac{1}{2}$ the log. of 50 from $\frac{1}{2}$ the log. of 120, run the eye along the tables of logarithmic tangents and ſecants until an arc be found of which the ſum of the log. tangent and half the log. ſecant is equal to 19.7653631, the remainder.

In

In this manner it will be readily found, that the ſum of the log. tangent and half the log. ſecant of 28° 37′ is leſs than that difference by 2012, and that the ſum of the log. tangent and half the log. ſecant of 28° 38′ is greater than it by 1337: therefore 3349 (2012 + 1337) : 60″ :: 2012 : 36″. The exact arc, therefore, of which the ſum of the log. tangent and half the log. ſecant is equal to 19.7653631 is 28° 37′ 36″, and the log. ſecant of it is 10.0566242, which being increaſed by 0.8494850, the log. of $\sqrt{50}$ gives 0.9061092, which is the logarithm of 8.055810, the value of x ſought, and which is true to ſeven places of figures.

EXAMPLE II.

To find the value of x in an equation of the form $x^3 - r^2x = -a$.

If r be expounded by 3, and a by 10, as they are in the example, given at p. 433. of the Phil. Tranſ. vol. LXX. the equation will be $x^3 - 9x = -10$, and may be transformed to $\sqrt{x} \times \sqrt{9 - x^2} = \sqrt{10}$; and, therefore, by tab. I. the ſquare root of the ſine into the coſine of an arc, of which the radius is 3, is equal to the ſquare root of 10. Conſequently, an arc muſt be found, ſuch that the ſum of the log. coſine and half the log. tangent is equal to half the log. of 10. But becauſe the radius of this arc muſt be 3, the log. ſines and coſines muſt be increaſed by the log. of 3; and, therefore, log. coſ. + log. of 3 + ½ log. ſine + ½ log. of 3 muſt be equal to half the log. of 10; or, an arc muſt be found of which the ſum of the tabular log. coſine and half the log. ſine is equal to the difference between half the log. of 10 and 1½ the log. of 3. Hence, having ſubtracted 1½ log. of 3 from half the log. of 10, run the eye

along GARDINER's tables of logarithmic ſines, by which means it will be readily found, that the ſum of the log. coſine and half the log. ſine of 28° 53′ 30″ is leſs than 19.7843181, the excels of half the log. of 10 above 1½ log. 3, by 15, and that the ſum of the log. coſine and half the log. ſine of 28° 53′ 40″ is greater than that difference by 60. Conſequently 75 (15 + 60) : 10″ :: 15 : 2″. The exact arc, therefore, of which the ſum of the log. coſine and half the log. ſine is equal to 19.7843181, is 28° 53′ 32″; and the log. ſine of this arc, increaſed by the log. of 3, is 0.1612153, the logarithm of 1.44949, the value of x required, true to the laſt place.

But many equations of this form, and this example among the reſt, admit of two poſitive values of the unknown quantity; and by carrying the eye farther along the tables it will be found alſo, that the ſum of the log. coſine and half the log. ſine of 41° 48′ 30″ is greater than 19.7843181 by 50, and that the ſum of the log. coſine and half the log. ſine of 41° 48′ 40″ is too little by 21. Conſequently, 71 (50 + 21) : 10″ :: 50 : 7″: of courſe, 41° 48′ 37″ is another arc, of which the ſum of the log. coſine and half the log. ſine is equal to 19.7843181, and the log. ſine of this arc, increaſed by the log. of 3, is the logarithm of 1.999999, another value of x, and which errs but by unity in the ſeventh place.

The third root, as it is generally called, of this equation, which is neceſſarily negative, and equal to the ſum of the other two, belongs properly to the equation which is given as the firſt example, of which it is the affirmative root, and may be found by the directions which are there given.

EXAMPLE

EXAMPLE III.

To find the value of x in an equation of the form $x^3 + r^2x = a$.

Let us take as examples of this equation $x^3 + 3x = .04$, $x^3 + 3x = .08$, and $x^3 + 3x = .12$, which are three of the inſtances given by Dr. HALLEY, in his Synopſis of the Aſtronomy of Comets, to illuſtrate the mode of computation that he purſued in conſtructing his general table for calculating the place of a comet in a parabolic orbit: and it is obvious, a being put for the known ſide of the equation, that it may be transformed to $\sqrt{x} \times \sqrt{3 + x^2} = \sqrt{a}$: where, if x be conſidered as the tangent of an arc, the radius of which is $\sqrt{3}$, $\sqrt{3 + x^2}$ will be the ſecant of that arc; and, conſequently, by what is ſhewn in the firſt example, an arc muſt be found, ſuch, that the ſum of the tabular log. ſecant and half the tabular log. tangent may be equal to the exceſs of half the log. of a above $\frac{1}{4}$ of the log. of 3. In the firſt of the above three inſtances this exceſs will be found, 18.9431891, in the ſecond 19.0937041, and in the third 19.1817497; and by running the eye along GARDINER'S Tables of Logarithmic Sines and Tangents, it will be found, that the firſt falls between 0° 26′ 20″ and 0° 26′ 30″, the ſecond between 0° 52′ 50″ and 0° 53′ 0″, and the third between 1° 19′ 20″ and 1° 19′ 30″; and, by purſuing the mode which has been deſcribed in the two former examples, the exact arcs will be found 0° 26′ 27″,7, 0° 54′ 51″,7, and 1° 19′ 20″,1, and their reſpective tangents, to the radius $\sqrt{3}$, .01333248, .0266611, and .0399787, the three values of x ſought. And in this manner Dr. HALLEY's table may be extended to any length with the utmoſt eaſe, expedition, and accuracy.

Thus far this matter has been carried by former writers; but thoſe who may be at the trouble of conſulting them will find that I have not copied their methods: on the contrary, theſe which are given here are more plain and obvious than theirs are, and the operations conſiderably ſhorter. What follows has not, I believe, been adverted to by any before me.

EXAMPLE IV.

Let the equation ariſing from the proportion $a : b+x \,.\, \overline{1-c^2} :: c\sqrt{1-x^2} : c^2x$ be taken, which is the reſult of an inquiry into the ſituation of that place on the ſurface of the earth, conſidered as a ſpheroid, which is at the greateſt diſtance from a given one, ſuppoſe London. In this inquiry a and b were put to repreſent the ſine and coſine of the latitude of the given place (in the ſpheroid); c for $\frac{229}{230}$, the ratio of the axes; and x for the ſine of the diſtance of the required place from the oppoſite pole (in the ſpheroid alſo). The equation, which is of four dimenſions with all the terms, is manifeſtly $acx = \overline{b+x \,.\, 1-c^2} \times \sqrt{1-x^2}$, or $\frac{x}{\sqrt{1-x^2}} - x \,.\, \frac{1-c^2}{ac} = \frac{b}{ac}$; in which it is evident from tab. I. that the difference between the tangent and the product of the ſine into a given quantity is known. In order, therefore, to find the value of x, compute $\frac{b}{ac}$, and $\frac{1-c^2}{ac}$, and find the logarithm of the latter. Now, becauſe the elliptic meridian differs but little from a circle, the place ſought will not be far from the antipodes of the given one, and its diſtance from the oppoſite pole may therefore be eſtimated at $39^\circ\ 5'$;

39° 5′; and, having taken out the natural tangent, and logarithmic ſine of this arc, add the logarithm of $\frac{1-e^2}{ac}$ to the latter, and find the number correſponding to the ſum, which will be leſs than the natural tangent of 39° 5′ by 2869. As this aſſumption is ſo near, take 39′ 6″ for the next, repeat the operation, and the reſult will be 1935 too great. Then 4804 (2869 + 1935) : 60″ :: 2869 : 36″; which being added to 39°5′, gives 39° 5′ 36″, for the co-latitude of the place ſought, and the natural ſine of this arc, or .6305856 is the value of x in this equation.

EXAMPLE V.

Let the equation $x^3 + \frac{b^2-2a^2}{4a}x^2 + \frac{2a^2-b^2}{2}x - \frac{ab^2}{4} = 0$, be taken, which reſults from a ſolution of one caſe of the problem *de inclinationibus* of APOLLONIUS; but which, as it naturally riſes to a ſolid problem, was not, I conceive, conſidered by that celebrated author. The reſult of the analyſis, before any reduction takes place, is this proportion, $x+a : x-a :: 2\sqrt{ax} : b$; and hence, $\frac{x-a}{x+a}\sqrt{ax} = \frac{1}{2}b$. But it is here manifeſt, that if a be taken for the tangent of an arc, of which the radius is $\sqrt{ax}$, x will be the cotangent of it, and $\frac{x-a}{x+a}\sqrt{ax} (= \frac{1}{2}b)$ the coſine of twice that arc. Conſequently, we have to find an arc, the tangent of which is to the coſine of twice that arc as a is to $\frac{1}{2}b$; and this being done, the natural co-tangent of that arc, to the proper radius, will be the value of x.

Thus, let a be 10, and b 24; and the difference of the logarithms of a and $\frac{1}{2}b$ will be 0.0791812. Now, by running the eye along GARDINER's Tables of logarithmic Sines and Tangents, it will be readily ſeen, that the log. tangent of 26° 33′ 50″, when increaſed by 0.0791812, is leſs than the coſine of 53° 7′ 40″ twice that arc; and that the log. tangent of 26° 34′ 0″, when increaſed by the ſame quantity, is too great. And, by actually taking out the logarithms, and making the additions, the former will be found too ſmall by 455, and the latter too great by 632. Then, 1087 (455 + 632) : 10″ :: 455 : 4″; which being added to 26° 33′ 50″ gives 26° 33′ 54″ for the arc of which x is the co-tangent. And if to twice the log. co-tangent of this arc the logarithm of a (10) be added, the ſum (1.6020600) is the log. of 40, the value of x ſought.

EXAMPLE VI.

The equation reſulting from a ſolution of the famous problem of ALHAZEN may be given as another example of the uſe of this method. Many ſolutions of this celebrated problem, by HUYGENS, SLUSIUS, and others, may be met with in the Philoſophical Tranſactions. Solutions to it may alſo be found at the end of Dr. ROBERT SIMPSON's Conic Sections, in Dr. SMITH's Optics, Mr. ROBIN's Mathematical Tracts, and other places; but the moſt direct and obvious method is, perhaps, that which follows.

Put $a = DC$, $b = dC$, $r = CI$; $x = CB$ and $y = CE$, the coſines of the arcs IA, IH, to the radius r: then will the ſines of thoſe arcs, BA, EH, be expreſſed by $\sqrt{r^2 - x^2}$ and $\sqrt{r^2 - y^2}$;

and,

and, because of the similar triangles ABC and BFC, HEC and dGC, $r : x :: a : \frac{ax}{r} = \text{CE}$, $r : \sqrt{r^2 - x^2} :: a : \frac{a\sqrt{r^2 - x^2}}{r} = \text{DF}$; $r : y :: b : \frac{by}{r} = \text{CG}$, and $r : \sqrt{r^2 - y^2} :: b : \frac{b\sqrt{r^2 - y^2}}{r} = d\text{G}$; consequently, $\frac{ax}{r} - r = \text{FI}$, $\frac{bx}{r} - r = \text{GI}$; and because the angles DIF, dIG, are equal by the nature of the problem, and the angles DFI and dGI both right angles, the triangles DFI and dGI are also similar, and consequently $\frac{a\sqrt{r^2 - x^2}}{r} : \frac{ax}{r} - r :: \frac{b\sqrt{r^2 - y^2}}{r} : \frac{by}{r} - r$; and $\frac{x}{\sqrt{r^2 - x^2}} - \frac{r^2}{a\sqrt{r^2 - x^2}} = \frac{y}{\sqrt{r^2 - y^2}} - \frac{r^2}{b\sqrt{r^2 - y^2}}$; or $\frac{r^2}{b\sqrt{r^2 - y^2}} - \frac{r^2}{a\sqrt{r^2 - x^2}} = \frac{y}{\sqrt{r^2 - y^2}} - \frac{x}{\sqrt{r^2 - x^2}}$: or, the co-secant of the arc HI $\div b$ – co-secant of AI $\div a$ = the co-tangent HI $\div r$ – co-tangent AI $\div r$; or, lastly, the co-tangent of HI – co-tangent of AI = co-secant of HI $\times \frac{r}{b}$ – co-secant AI $\times \frac{r}{a}$. Consequently, we have to find two arcs, the sum of which is given, and such that the difference of their co-tangents may be equal to the difference of the products of their co-secants into given quantities.

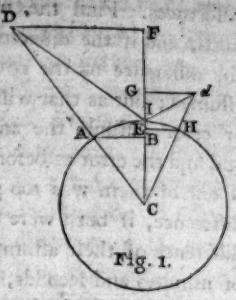


Fig. 1.

To do this assume the angle DCF as near as possible; and, because the sum of the two angles is given, the angle dCG will be known also. Take the difference of the logarithms of r and a, r and b, which will be constant, also the difference of the co-tangents of the two assumed arcs, and having taken out the log. co-secants, add to them respectively the two logarithmic differences.

differences. Find the numbers correſponding to theſe two ſums, and if the difference of theſe two numbers be equal to the difference of the co-tangents, the angle DCF was rightly aſſumed; but as that will ſeldom happen, take the difference, or error; aſſume the angle DCF again, repeat the operation, and find the error as before. Then, as the ſum of the errors, if one of them was too great, and the other too little, or their difference, if both were too great, or both too little, is to the difference of theſe aſſumptions, ſo is the leſs error to a number of minutes and ſeconds, which muſt be added to that aſſumption to which the leaſt error belongs, if that aſſumption was too ſmall; or ſubtracted from it, if the aſſumption was too great: and, unleſs the firſt aſſumption was made very wide of the truth, which may always be avoided, the two angles will generally be obtained within a few ſeconds of the truth, and, by repeating the operation once more, to the utmoſt exactneſs.

Suppoſe DC (a) be taken equal to 72, dC (b) = 48, and the radius CI (r) = 40, the angle DCd being 82° 45′: then the whole operation will ſtand as follows:

r = 40 log.	11.6020600	- -	11.6020600
a = 72 log.	1.8573325	b = 48 log.	1.6812412
Conſtant log.	9.7446275	Conſtant log.	9.9208188

Now, in the two triangles DCI, dCI, the angles DIC and dIC being equal, and CI common, but dC conſiderably leſs than DC, it is manifeſt, that the angle dCI will be conſiderably leſs than the angle DCI: let them be aſſumed in the proportion that DC bears to its exceſs above dC; in which caſe the angle dCI will be 27° 35′ and DCI 55° 10′. The co-tangent of the former will be 1.9141795, of the latter .6958813; and the difference of them 1.2182982. The log. co-ſecants of thoſe two

two angles are 10.3343832 and 10.0857536, which being reſpectively increaſed by 9.9208188 and 9.7446275, the two conſtant logarithms, make 0.2552020 and 9.8304811, which are the logarithms of 1.7997079 and .6768323; and the difference of theſe two numbers is 1.1228756, which is leſs than the difference of the log. co-tangents by .0954226.

I next aſſume the angles 30° and 52° 45′; and by purſuing the ſame ſteps which have been deſcribed above, I find the difference of their co-tangents exceeds the difference of the products by .0028987. Then, as 925239 (the difference of the errors) is to 145′ (the difference of ſuppoſitions), ſo is the latter error 28987 to 4′ 33″, which being added to 30°, gives 30° 4′ 33″ for the next aſſumption of the angle *d*CI; but for eaſe in the computation I ſhall take 30° 5′; in which caſe the angle DCI will be 52° 40′; and by repeating the operation the difference of the co-tangents will be found leſs than the difference of the products by .0002425. And 31412 (the ſum of the two laſt errors) is to 5′ (the difference of the ſuppoſitions) as 2425 (the laſt error) is to 23″; which being taken from 30° 5′, the laſt ſuppoſition, becauſe it was too great, leaves 30° 4′ 37″ for the exact value of the angle *d*CI.

This equation, like that in the fourth example, when the value of *y* is properly ſubſtituted, and the equation reduced in the uſual manner, will riſe to four dimenſions with all the inferior ones; and it does not appear, that either HUYGENS, SLUSIUS, Mr. ROBINS, Dr. WILLSON, or Profeſſor SIMSON, with all their artifice, have been able to depreſs it: but by this method of reſolution the point of reflection is found, with the greateſt exactneſs, in much leſs time than this ſubſtitution and reduction can be made. And this example farther ſuggeſts to us, that when the anſwer is ſought by the method now under

conſideration, it is not always neceſſary to exterminate all the unknown quantities but one.

EXAMPLE VII.

Suppoſe the equation to be reſolved were $a = .375 = 16y^5 \mp 4y^4 - 20y^3 \pm 4y^2 + 5y$: and, firſt, let the upper ſigns have place, and it is manifeſt, that the latter ſide of the equation may be divided into two parts; namely, $4y^2 - 4y^4 = 4y^2 \cdot \overline{1+y} \cdot \overline{1-y}$, and $16y^5 - 20y^3 + 5y = y^5 - 10y^3 \cdot \overline{1+y} \cdot \overline{1-y} + 5y \cdot \overline{1+y}^2 \cdot \overline{1-y}^2$. But the former part is (by tab. I.) the ſquare of the ſine of twice the arc which has y for its ſine (radius being $= 1$) and the latter part the ſine of five times the ſame arc. Hence, therefore, the given quantity a $(= .375)$ is equal to the ſum of the ſine of five times the arc (A) which has y for its ſine, and the ſquare of the ſine of twice the ſame arc. Now, as the ſquare of the ſine of twice the arc (A) muſt neceſſarily in this inſtance be very ſmall in compariſon of the ſine of five times the ſame arc (A), it is manifeſt, that the ſine of five times the arc which has y for its ſine will be very little leſs than .375, and of courſe that arc (5A) can be but very little leſs than 22° 2′, the ſine of which is next greater than that number. Aſſume it 21°, and the fifth part of it, or that arc which has y for its ſine, will be 4° 12′, the double of which is 8° 24′. Now the log. ſine of 8° 24′ is 9.1645998, which being doubled is 8.3291996, the logarithm of .0213403, and this number being taken from .375 leaves .3536597, which ought to have been .3583679, the ſine of 21°, and of courſe is too ſmall by .0047082: the arc has, therefore, been aſſumed too great.

Let

Let 20° 45′ be next aſſumed; the fifth part of which is 4° 9′, and twice this laſt number is 8° 18′, of which the log. ſine is 9.1594354; and this being doubled is 8.3188708, the log. of .0208387; and this being taken from .375 will leave .3541613: leſs ſtill than the ſine of 20° 45′ by .0001297.

Take now 20° 44′, the fifth of which is 4° 8′ 48″, and two-fifths is 8° 17′ 36″; and the log. ſine of this is 9.1590889, which being doubled gives 8.3181778, the logarithm of .0208055; and this being taken from .375, leaves .3541945; more than the ſine 20° 44′ by .0000795. Now 2092 (the ſum of the laſt two laſt errors) is to 60″ as 795 (the laſt error) is to 23″. Which being added to 20° 44′, the laſt aſſumption, gives 20° 44′ 23″ for five times the arc of which *y* is the ſine: *y* is therefore the ſine of 4° 8′ 52″.6, or .07233202.

When the lower ſigns in the equation have place, the given quantity *a* will be equal to the exceſs of the ſine of five times an arc above the ſquare of the ſine of twice that arc: and the operation, after aſſuming, from the circumſtances of the queſtion, or equation, an arc which is nearly five times that having *y* for its ſine, is this. Find the logarithmic ſine of two-fifths of that arc, double it, find the number correſponding to this logarithm, and add to it the value of *a*, which ſhould then be equal to the ſine of the arc firſt aſſumed; and if it is not, to repeat the operation until an error is obtained on each ſide, and not very diſtant from the truth, as is done above, and which may always be done with three aſſumptions.

A multitude of examples might be added from the writings of different authors, who have either left their concluſions

unexhibited in numbers, for want of ſome ſuch eaſy method as this, or have done it by means of a long and laborious ſeries of difficult computations; which, beſide the labour attending them, are always ſubject to a variety of errors, which cannot be detected, in many caſes, without repeating the operation.

XXXI. *Experiments on the Power that Animals, when placed in certain Circumſtances, poſſeſs of producing Cold.* By Adair Crawford, *M. D.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 14, 1781.

IN the following paper I ſhall lay before the Society the reſult of ſome experiments, which I made in the courſe of the laſt ſummer, on the power that animals, when placed in certain circumſtances, poſſeſs, of producing cold, having premiſed a few remarks on the progreſſive improvements which have been made in the knowledge of heat in general.

The opinions of the ancients, reſpecting the nature and properties of fire, conſiſted of bold conjectures, which ſeem rather to have been the offspring of a lively and vigorous imagination, than of a juſt and correct judgement: their ideas on this ſubject being evidently derived, not ſo much from an accurate obſervation of facts, as from thoſe ſentiments of admiration and awe which many of the phenomena of fire are calculated to excite. Thus, this element was ſuppoſed, on the original formation of the univerſe, to have aſcended to the higheſt place, and to have occupied the region of the heavens; it was conceived to be the principle which firſt communicated life and activity to the animal kingdom: it was conſidered as conſtituting the eſſence of inferior intellectual beings; and, by many of the ancient nations, it was reverenced as the ſupreme Deity. Indeed the profound veneration with which the element of

fire was contemplated, for a long ſucceſſion of ages, by a great part of mankind, appears to be one of the moſt curious circumſtances in the hiſtory of antient opinions. To account for this we may obſerve, that there is no principle in nature, obvious to the ſenſes, which produces ſuch important effects in the material ſyſtem, and which, at the ſame time, in the mode of its operation, is ſo obſcure and incomprehenſible.

It appears to be accumulated in an immenſe quantity in the ſun and fixed ſtars, from whence its beneficial influence ſeems to be continually diffuſed over the univerſe: it is the great inſtrument by means of which the changes of the ſeaſons are effected; the diverſity of climates is chiefly owing to the various proportions in which it is diſtributed throughout the earth. If we add to this the mighty alterations which have been produced in human affairs by the introduction of artificial fire, by its employment in the ſeparation of metals from their ores, and in the various arts which are ſubſervient to the comfort, the ornament, and the preſervation, of the ſpecies, it will not appear ſurprizing, that in a rude and ignorant age, this wonderful principle ſhould have been conſidered as endued with life and intelligence, and that it ſhould have become the object of religious veneration.

In the dark ages the alchymiſts regarded pure fire as the reſidence of the Deity: they conceived it to be uncreated and immenſe, and attributed to its influence moſt of the phenomena of nature. Indeed, it is not wonderful that they ſhould have aſſigned it a high rank in the ſcale of being, as it was the great agent which they employed in the chymical analyſis of bodies, and was the inſtrument of thoſe diſcoveries that attracted ſuch univerſal admiration, and that enabled them ſo ſucceſſfully to impoſe upon the ignorance and credulity of the times.

Upon

Upon the revival of literature, the importance of this branch of ſcience began very ſoon to engage the attention of philoſophers. It could not eſcape the general obſervation, in a penetrating and inquiſitive age, when the powers of the human mind were employed with ſo much ardour and ſucceſs in exploring the operations of nature, that the element of fire acts a principal part in the ſyſtem of the world; that by the influence of this element thoſe motions are begun and ſupported in the animal and vegetable kingdoms, which are eſſential to the production and preſervation of life; and that it is the great agent in thoſe ſucceſſive combinations and decompoſitions, by which all things on the ſurface of the earth, and probably throughout the univerſe, are kept in a continual fluctuation.

But though the utility of this branch of ſcience was perceived, yet the progreſs that was made in the cultivation of it did not keep pace with the opinion which men entertained of its importance. Our ſenſes inform us, that heat has a real exiſtence, but they give us no direct information with regard to its nature and properties: it is endowed with ſuch infinite ſubtilty, that it has been called, by a very eminent philoſopher, an occult quality: by ſome it has even been conſidered as an immaterial being. It is, therefore, with great difficulty that it can be made the ſubject of philoſophical inveſtigation; and hence the opinions of men concerning it have been fluctuating and various, and the words which expreſs it vague and ambiguous.

The firſt ſtep that was taken with a view to the cultivation of this branch of ſcience was the conſtruction of a machine for meaſuring the variations of ſenſible heat; obſerving, that heat has the power of expanding bodies, and conſidering

the degree of expanſion as proportional to the increaſe of heat, philoſophers have endeavoured by means of the former to render the latter obvious to the ſenſes.

To this important invention, the author of which cannot be diſtinctly traced, we are indebted for all the ſucceeding improvements in the philoſophy of heat. By means of it men were enabled to eſtabliſh a variety of intereſting facts, and to bring ſome of the moſt obſcure and intricate phenomena of nature to the teſt of experiment. The opinion, that the heats inherent in various heterogeneous ſubſtances differed from each other in kind, as well as in degree, was now exploded, ſince all were found to produce ſimilar effects upon the thermometer. The increaſe and diminution of temperature in the different ſeaſons and climates, the laws which nature obſerves in the heating and cooling of bodies, the melting, the vaporific, and ſhining points, and the degrees of heat in the animal, the mineral, and the vegetable kingdoms, were accurately determined. In conſequence of the attention that was paid to this ſubject, many curious queſtions aroſe, which have long exerciſed the ingenuity of philoſophers. That property of heat by which it is capable of expanding the denſeſt and hardeſt bodies; its power in producing fluidity; its tendency to an equilibrium; and the cauſes of its various diſtribution throughout the different ſubſtances in nature, have become the objects of philoſophical enquiry. It was obſerved, that ſome bodies on expoſure to heat, become red and luminous, but are incapable of producing flame, or of maintaining fire: that, on the contrary, others, by the application of fire, and the contact of freſh air, kindle into flame, and continue to emit light and heat, apparently from a ſource within themſelves, till they are conſumed. Hence aroſe the queſtions concerning the pabu-

lum

ſum of fire, the uſe of the air in inflammation, and the diſtinction of bodies into combuſtible and incombuſtible.

From the firſt dawnings of philoſophy it muſt have been perceived, that moſt animals have a higher temperature than the medium in which they live; and that a conſtant ſucceſſion of freſh air is neceſſary to the ſupport of animal life. The cauſes of theſe phenomena have afforded matter for much ſpeculation in ancient as well as modern times: but the diſcovery that animals have, in certain circumſtances, the power of keeping themſelves at a lower temperature than the ſurrounding medium, was reſerved for the induſtry of the preſent age.

This diſcovery ſeems originally to have ariſen from obſervations on the heat of the human body in warm climates. It was mentioned by Governor ELLIS in 1758; it was taught by Dr. CULLEN before the year 1765; and at length it was completely eſtabliſhed by the experiments of Dr. FORDYCE in heated rooms, which were laid before the Society in 1774.

In the courſe of theſe experiments the doctor remained in a moiſt air heated to 130° for the ſpace of fifteen minutes, during which time the thermometer under his tongue ſtood at 100°, his pulſe made 139 beats in a minute, his reſpiration was but little affected, and ſtreams of water ran down over his whole body, proceeding from the condenſation of vapour, as evidently appeared from a ſimilar condenſation on the ſide of a Florentine flaſk that had been filled with water at 100°.

He found, however, that he could bear a much greater degree of heat when the air was dry. In this ſituation, he frequently ſupported, naked, for a conſiderable time, without much inconvenience, the heat of 260°, his body preſerving very nearly its proper temperature, being never raiſed more than 2° above the natural ſtandard.

Various

Various opinions have been entertained with regard to the cauſes of the facts which were eſtabliſhed by theſe experiments. Some have attributed the cold ſolely to evaporation, and have conceived that the ſame degree of refrigeration would have been produced by an equal maſs of dead matter, containing an equal quantity of moiſture. Others have affirmed, that the cold did not ariſe ſolely from this cauſe; but have maintained, that it depended partly upon the energy of the vital principle, being greater than what would have been produced by an equal maſs of inanimate matter.

The ingenious Dr. MUNRO, of Edinburgh, aſcribes the cold in the above mentioned experiments to the circulation of the blood, in conſequence of which the warmer fluids are continually propelled from the ſurface towards the center, where they are mixed with blood at a lower temperature, and hence the animal is ſlowly heated, in the ſame manner as the water in a deep lake, during the winter, is ſlowly cooled, and not without a long continuance of froſt congealed, no part of it becoming ſolid till the whole is brought down to the freezing point.

The following experiments were made with a view to determine with greater certainty the cauſes of the refrigeration in the above inſtances.

To diſcover whether the cold produced by a living animal, placed in air hotter than its body, be not greater than what would be produced by an equal maſs of inanimate matter, I took a living and a dead frog, equally moiſt, and of nearly the ſame bulk, the former of which was at 67°, the latter at 68°, and laid them upon flannel in air which had been raiſed to 106°. In the courſe of twenty-five minutes the order of heating was as follows *.

* In the two following experiments the thermometers were placed in contact with the ſkin of the animals under the axillæ.

In

		Air.	Dead frog.	Living frog.
		°	°	°
In	1	—	70½	67½
	2	102	72	68
	3	100	72½	69½
	4	100	73	70
	25	95	81½	78½

The thermometer being introduced into the ſtomach, the internal heat of the animals was found to be the ſame with that at the ſurface.

From hence it appears, that the living frog acquired heat more ſlowly than the dead one. Its vital powers muſt, therefore, have been active in the generation of cold.

To determine whether the cold produced in this inſtance depended ſolely upon the evaporation from the ſurface, increaſed by the energy of the vital principle, a living and dead frog were taken at 75°, and were immerſed in water at 93°, the living frog being placed in ſuch a ſituation as not to interrupt reſpiration.

		Dead frog.	Living frog.
		°	°
In	1	85	81
	2	88½	85
	3	90½	87
	5	91½	89
	6	91½	89
	8	91½	89

Theſe experiments prove, that living frogs have the faculty of reſiſting heat, or producing cold, when immerſed in warm water: and the experiments of Dr. FORDYCE prove, that the human body has the ſame power in a moiſt as well as in a dry

* In the above experiment the water, by the cold frogs and by the agitation which it ſuffered during their immerſion, was reduced nearly to 91°½.

air:

air: it is therefore highly probable, that this power does not depend ſolely upon evaporation.

It may not be improper here to obſerve, that healthy frogs, in an atmoſphere above 70°, keep themſelves at a lower temperature than the external air, but are warmer internally than at the ſurface of their bodies: for when the air was 77°, a frog was found to be 68°, the thermometer being placed in contact with the ſkin; but when the thermometer was introduced into the ſtomach, it roſe to 70°¼.

It may likewiſe be proper to mention, that an animal of the ſame ſpecies placed in water at 61°, was found to be nearly 61°¼ at the ſurface, and internally it was 66°¼. Theſe obſervations are meant to extend only to frogs living in air or water at the common temperature of the atmoſphere in ſummer. They do not hold with reſpect to thoſe animals, when plunged ſuddenly into a warm medium, as in the preceding experiments.

To determine whether other animals alſo have the power of producing cold, when ſurrounded with water above the ſtandard of their natural heat, a dog at 102° was immerſed in water at 114°, the thermometer being cloſely applied to the ſkin under the axilla, and ſo much of his head being uncovered as to allow him a free reſpiration.

In 5 minutes the dog was 108°, water 112°
6 - - 109 - 112
11 - - 108 - 112 the reſpiration having become very rapid.

In thirteen minutes the dog was 108°, water 112°, the reſpiration being ſtill more rapid.

In about half an hour the dog was 109°, water 112°, the animal was then in a very languid ſtate.

Small

Small quantities of blood being drawn from the femoral artery, and from a contiguous vein, the temperature did not ſeem to be much increaſed above the natural ſtandard, and the ſenſible heat of the former appeared to be nearly the ſame with that of the latter.

In this experiment a remarkable change was produced in the appearance of the venous blood: for it is well known, that in the natural ſtate, the colour of the venous blood is a dark red, that of the arterial being light and florid; but after the animal, in the experiment in queſtion, had been immerſed in warm water for half an hour, the venous blood aſſumed very nearly the hue of the arterial, and reſembled it ſo much in appearance, that it was difficult to diſtinguiſh between them. It is proper to obſerve, that the animal which was the ſubject of this experiment, had been previouſly weakened by loſing a conſiderable quantity of blood a few days before. When the experiment was repeated with dogs which had not ſuffered a ſimilar evacuation, the change in the colour of the venous blood was more gradual; but in every inſtance in which the trial was made, and it was repeated ſix times, the alteration was ſo remarkable, that the blood which was taken in the warm bath could readily be diſtinguiſhed from that which had been taken from the ſame vein before immerſion, by thoſe who were unacquainted with the motives or circumſtances of the experiment.

To diſcover whether a ſimilar change would be produced in the colour of the venous blood in hot air, a dog at 102° was placed in air at 134°.

In ten minutes the temperature of the dog was 104°4, that of the air being 130. In fifteen minutes the dog was 106°, the air 130°. A ſmall quantity of blood was then taken from the

jugular vein, the colour of which was ſenſibly altered, being much lighter than in the natural ſtate.

The effect which is produced by external heat upon the colour of the venous blood, ſeems to confirm the following opinion, which was firſt ſuggeſted by my worthy and ingenious friend Mr. WILSON, of Glaſgow. Admitting that the ſenſible heat of animals depends upon the ſeparation of abſolute heat from the blood by means of its union with the phlogiſtic principle in the minute veſſels, may there not be a certain temperature at which that fluid is no longer capable of combining with phlogiſton, and at which it muſt of courſe ceaſe to give off heat? It was partly with a view to inveſtigate the truth of this opinion that I was led to make the experiments recited above.

I ſhall now endeavour, from the preceding facts, to explain what appear to me to be the true cauſes of the cold produced by animals when placed in a medium, the temperature of which is above the ſtandard of their natural heat.

In a work which I ſome time ago laid before the public, having attempted to prove, that animal heat depends upon the ſeparation of elementary fire from the air in the proceſs of reſpiration, I obſerved, that when an animal is placed in a warm medium, if the evaporation from the lungs be increaſed to a certain degree, the whole of the heat ſeparated from the air will be abſorbed by the aqueous vapour.

From the experiments on venous and arterial blood, recited in the third ſection of that work, it appears, that the capacity of the blood for containing heat is ſo much augmented in the lungs, that, if its temperature were not ſupported by the heat which is ſeparated from the air, in the proceſs of reſpiration, it would ſink 30°. Hence, if the evaporation from the lungs be ſo much increaſed as to carry off the whole of the heat that is detached

detached from the air, the arterial blood when it returns by the pulmonary vein will have its ſenſible heat greatly diminiſhed, and will conſequently abſorb heat from the veſſels which are in contact with it, and from the parts adjacent. The heat which is thus abſorbed in the greater veſſels will again be extricated in the capillaries, where the blood receives a freſh addition of phlogiſton. If, in theſe circumſtances, the blood during each revolution were to be equally impregnated with this latter principle, it is manifeſt, that the whole effect of the above proceſs would be to cool the ſyſtem at the center, and to heat it at the ſurface; or to convey the heat to that part of the body where it is capable of being inſtantly carried off by evaporation. But it appears, from the experiments which have been laſt recited, that, when an animal is placed in a heated medium, the ſanguineous maſs, during each revolution, is *leſs* impregnated with phlogiſton; for we have ſeen, that the venous blood, in theſe circumſtances, becomes gradually paler and paler in its colour till at length it acquires very nearly the appearance of the arterial: and it is rendered highly probable by the experiments of Dr. PRIESTLEY, that the dark and livid colour of the blood in the veins depends upon its combination with phlogiſton in the minute veſſels. Since, therefore, in a heated medium, this fluid does not aſſume the ſame livid hue, we may conclude, that it does not attract an equal quantity of the phlogiſtic principle *.

* It is of no conſequence in the above argument, whether we ſuppoſe, with Dr. PRIESTLEY, that the alteration of colour in the blood depends upon its combination with phlogiſton in the capillary arteries, or maintain with ſome other philoſophers that this alteration ariſes from a change produced in the blood itſelf by the action of the veſſels; it is ſufficient for our purpoſe to aſſume it as a fact, which, I think, has been proved by direct experiment, that, in the natural ſtate of the animal, the blood undergoes a change in the capillaries, by which its capacity for containing heat diminiſhed; and that in a heated medium it does not undergo a ſimilar change.

It follows, that the quantity of heat given off by the blood in the capillaries will not be equal to that which it had absorbed in the greater veſſels, or poſitive cold will be produced. If the blood, for example, in its paſſage to the capillaries, abſorb from the greater veſſels a quantity of heat as 30°, and if in conſequence of its receiving a leſs impregnation of phlogiſton than formerly, it give off at the extreme veſſels a quantity of heat only as 20°, it is manifeſt, that upon the whole a degree of refrigeration will be produced as 10°, and this cauſe of refrigeration will continue to act while the venous blood is gradually aſſuming the hue of the arterial, till the difference between them is obliterated; after which it will ceaſe to operate. Thus it appears, that when animals are placed in a warm medium, the ſame proceſs which formerly ſupplied them with heat becomes for a time the inſtrument of producing cold, and probably preſerves them from ſuch rapid alterations of temperature as might be fatal to life.

Upon the whole, the increaſed evaporation, the diminution of that power by which the blood in the natural ſtate is impregnated with phlogiſton, and the conſtant reflux of the heated fluids towards the internal parts, ſeem to be the great cauſes upon which the refrigeration depends. Having found that the attraction of the blood to phlogiſton was diminiſhed by heat, it appeared probable, on the other hand, that it would be increaſed by cold. To determine this, a dog at 100° was immerſed in water nearly at 45°. In about a quarter of an hour a ſmall quantity of blood was taken from the jugular vein, which was evidently much deeper in its colour than that which had been taken in the warm bath, and appeared to me, as well as to ſeveral other gentlemen, to be the darkeſt venous blood we had ever ſeen.

From this experiment, compared with thoſe which have been recited above, we may perceive the reaſon why animals preſerve an equal temperature, notwithſtanding the great variations in the heat of the atmoſphere, ariſing from the viciſſitudes of the weather, and the difference of ſeaſon and climate: for, as ſoon as by expoſure to external cold, an unuſual diſſipation of the vital heat is produced, the blood, in the courſe of the circulation, begins to be more deeply impregnated with the phlogiſtic principle. It will therefore furniſh a more copious ſupply of this principle to the air in the lungs, and will imbibe a greater quantity of fire in return.

In ſummer, on the contrary, the reverſe of this will take place, leſs phlogiſton will be attracted in the minute veſſels, and leſs fire will be abſorbed from the air.

And hence the power of generating heat is in all caſes proportioned to the demand. It is increaſed by the winter colds, diminiſhed by the ſummer heats: it is totally ſuſpended or converted into a contrary power, according as the exigencies of the animal may require.

From the changes which are produced in the colour of the venous blood by heat and cold, we may likewiſe perceive the reaſon why the temperature of the body is frequently increaſed by plunging ſuddenly into cold water, and why the warm bath has ſuch powerful effects in cooling the ſyſtem, and in removing a general or partial tendency to inflammation.

XXXII. *Account of a Comet. By Mr.* Herſchel, *F. R. S.; communicated by Dr.* Watſon, *Jun. of* Bath, *F. R. S.*

Read April 26, 1781.

ON Tueſday the 13th of March, between ten and eleven in the evening, while I was examining the ſmall ſtars in the neighbourhood of H Geminorum, I perceived one that appeared viſibly larger than the reſt: being ſtruck with its uncommon magnitude, I compared it to H Geminorum and the ſmall ſtar in the quartile between Auriga and Gemini, and finding it ſo much larger than either of them, ſuſpected it to be a comet.

I was then engaged in a ſeries of obſervations on the parallax of the fixed ſtars, which I hope ſoon to have the honour of laying before the Royal Society; and thoſe obſervations requiring very high powers, I had ready at hand the ſeveral magnifiers of 227, 460, 932, 1536, 2010, &c. all which I have ſucceſsfully uſed upon that occaſion. The power I had on when I firſt ſaw the comet was 227. From experience I knew that the diameters of the fixed ſtars are not proportionally magnified with higher powers, as the planets are; therefore I now put on the powers of 460 and 932, and found the diameter of the comet increaſed in proportion to the power, as it ought to be, on a ſuppoſition of its not being a fixed ſtar, while the diameters of the ſtars to which I compared it were not increaſed

in

in the ſame ratio. Moreover, the comet being magnified much beyond what its light would admit of, appeared hazy and ill-defined with theſe great powers, while the ſtars preſerved that luſtre and diſtinctneſs which from many thouſand obſervations I knew they would retain. The ſequel has ſhewn that my ſurmiſes were well founded, this proving to be the Comet we have lately obſerved.

I have reduced all my obſervations upon this Comet to the following tables. The firſt contains the meaſures of the gradual increaſe of the Comet's diameter. The micrometers I uſed, when every circumſtance is favourable, will meaſure extremely ſmall angles, ſuch as do not exceed a few ſeconds, true to 6, 8, or 10 thirds at moſt; and in the worſt ſituations true to 20 or 30 thirds: I have therefore given the meaſures of the Comet's diameter in ſeconds and thirds. And the parts of my micrometer being thus reduced, I have alſo given all the reſt of the meaſures in the ſame manner; though in large diſtances, ſuch as one, two, or three minutes, ſo great an exactneſs, for ſeveral reaſons, is not pretended to.

TABLE

TABLE I. Measures of the Comet's diameter *.

Days.	″ ‴	Power.	
March 17	2 53	932.	460.
19	2 59	932.	460.
21	3 38	460.	
28	4 7	932	these measures agree to 9‴.
—	3 58	227	
29	4 7	227	rather too small a measure.
—	4 25	227	seems right.
April 2	4 28	227	
6	4 53	227	
15	5 11	227	
—	5 20	227	very good: not liable to half a second of error.
18	5 2	227	true to 12‴ or 18‴ at most.

Having measured the diameter of the Comet with such high powers as 932 and 460, it may not be amiss to make one observation on this subject, lest it should be misapprehended that I pretend to a distinct power of such magnitude upon all celestial objects in general. By experience I have found, that the aberration or indistinctness occasioned by magnifying much, provided the object be still left sufficiently distinct, is rather to be put up with, than the power to be reduced, when the angles to be measured are extremely small. The reason of this may, perhaps, be that a small error of judgement, to which we are always liable, is of great consequence with a low power, as bearing a considerable proportion to the diameter of the object;

* There are several optical deceptions which may affect the measures of objects that subtend extremely small angles. Thus I have found, by experience, that a very small object will appear something less in a telescope when we see it first than when we become familiar with it. There is also a deflection of light upon the wires when they are nearly shut; but as none of these deceptions are well enough understood to apply a correction, I leave them affected with them.

whereas

whereas with a higher power the proportion of this error to the whole becomes much leſs, and the meaſure more exact, even after we have made allowance for a ſmall additional error occaſioned by the want of that perfect diſtinctneſs which is required for other purpoſes. However, to enter deeply into an explanation of this would lead me to ſpeak of the cauſes of the aberration of rays in the focus of an object ſpeculum, of which there are ſome that are ſeldom taken into conſideration by opticians, and indeed are ſuch as cannot be calculated; but this not being my preſent purpoſe, ſuffice it to obſerve, that the method is juſtified by experience.

When the diameter of the Comet was increaſed to about 4″, I thought it adviſable to leſſen the power with which I meaſured; and, as I made uſe of two different micrometers, as well as eye-glaſſes, I took a meaſure with both of them. The agreement of the micrometers to 9″ is no ſmall proof of the goodneſs of the obſervations of the 28th of March, and very properly connects the meaſures of the high powers with thoſe that were made with 227.

TABLE II. Distance of the Comet from certain teleſcopic fixed ſtars which I have marked α, β, γ, δ, ι, ζ.

D.	H.	M.		′	″	‴	
Mar. 13	10	30	from α,	2	48	0	by pretty exact eſtimation true to 20″.
17	11	0	fig. 1*.	0	41	58	by the micrometer and power 227.
18	7	20		1	0	35	
—	9	16		1	6	39	
—	10	55		1	10	40	
19	7	4		1	46	40	
—	10	42		1	51	23	
21	10	0		3	39	46	
24	8	12	from β,	2	55	39	true to 4 or 5″, an indifferent obſervation.
—	10	58	fig. 2.	2	53	4	true to 4 or 5″.
25	7	24		2	12	46	true to 2 or 3′.
—	9	47		2	14	18	
26	10	43		1	48	3	true to 2 or 3″.
28	7	46		2	55	49	true to 4 or 5′.
29	8	50	from γ.	2	20	51	true to 2″.
30	7	55	fig. 3.	1	28	48	true to 2 or 3″.
Apr. 1	7	45		2	39	20	
6	8	50	from δ. fig. 4	2	51	23	
15	10	18	from ι.	4	27	57	eſtimated by the field, true to 5 or 6″.
16	7	50	fig. 5.	3	9	14	by the micrometer, true to 3 or 4″.
—	10	47		2	50	56	true to 3 or 4″.
18	8	18		3	18	4	mean 3′ 17″, true to 1″ or 1½.
—	—	—		3	15	57	
—	8	50	from ζ,	2	24	57	
19	8	38	fig. 6.	3	2	5	true to 3 or 4″.

* The figures are drawn upon a ſcale of 80 ſeconds to one inch.

TABLE

TABLE III. Angle of position of the Comet with regard to the parallel of declination of the same telescopic fixed stars measured by a micrometer, of which I have given the description, and a magnifying power of 278. See fig. 1. 2. 3. 4. 5. 6.

D.	H.	M.		°	′	
Mar. 13	10	30	B α Comet,	0	0	by superficial estimation, liable to an error of 10 or 12 degrees.
17	11	0	A α Comet,	89	56	by the micrometer.
18	8	20	fig. 1 *.	56	39	
—	9	24		41	33	true to 1°.
19	7	23		29	47	true to 1°.
21	10	10		11	46	true to 4 or 5°.
—	11	48		12	14	
24	8	23	B β Comet,	38	39	true to 2 or 3°.
—	11	4	fig. 2.	36	14	true to 3 or 4°, air very tremulous.
25	7	33		53	18	
—	9	55		56	32	liable to a considerable error.
26	10	55	A β Comet.	87	0	true to 2 or 3°.
28	7	58		28	51	true to 3 or 4°.
29	9	25	B γ Comet,	32	19	true to 1 or 2°.
30	8	25	fig. 3.	72	14	true to 3 or 4°.
Apr. 1	7	55	A γ Comet,	28	51	well taken,
—	—	—		27	14	more exact, } 27° 46′, true to 1°.
6	8	28	B δ Comet, fig. 4.	84	42	true to less than 2°
15	10	27	B ε Comet,	29	9	true to 2 or 3°.
16	8	1	fig. 5.	49	11	true to 1°.
—	10	55		50	47	true to 1¼ or 2°¼.
18	8	31	A ε Comet,	47	9	very well taken,
—	—	—		46	35	pretty well, } 47°, true to less than 1°.
—	9	8	B ζ Comet,	82	39	
19	8	56	A ζ Comet,	48	18	
—	—	—	fig. 6.	49	48	} 49° 3′, true to 1°.
—	10	45		47	30	true to 2 or 3°.

* The angles are drawn true to the measure, without allowing for errors.

Miſcellaneous obſervations and remarks.

March 19. The Comet's apparent motion is at preſent 2¼ ſeconds *per* hour. It moves according to the order of the ſigns, and its orbit declines but very little from the ecliptic.

March 25. The apparent motion of the Comet is accelerating, and its apparent diameter ſeems to be increaſing.

March 28. The diameter is certainly increaſed, from which we may conclude that the Comet approaches to us.

April 2. This evening at 8 h. 15′ the Comet was a little above the line drawn from γ to θ in fig. 7. This figure is only delineated by the eye, ſo that no very great exactneſs in the diſtances of the ſtars is to be expected; but I ſhall take the firſt opportunity of meaſuring their reſpective ſituations by the micrometer.

April 6. With a magnifying power of 278 times the Comet appeared perfectly ſharp upon the edges, and extremely well defined, without the leaſt appearance of any beard or tail.

April 16. Fig. 8. repreſents the ſituation of the Comet this evening about nine o'clock, and is only an eye-draught of the teleſcopic ſtars.

Remarks on the path of the Comet.

We may obſerve, that the method of tracing out the path of a celeſtial body by taking its diſtance from certain ſtars, and the angle of poſition with regard to them, cannot be expected to give us a compleatly juſt repreſentation of the tract it deſcribes, ſince even the moſt careful obſervations are liable to little errors, both from the remaining imperfections of inſtruments, though they

they ſhould be the moſt accurate that can be had, and from the difficulty of taking angles and poſitions of objects in motion. Add to this a third cauſe of error, namely, the obſcurity of very ſmall teleſcopic ſtars that will not permit the field of view ſo well to be enlightened as we could wiſh, in order to ſee the threads of the micrometer perfectly diſtinct.

This will account for the apparent diſtortions to be obſerved in my figures of the Comet's path. Some little irregularity therein may alſo proceed from different refractions, as they have not been taken into account, though the obſervations have been made at very different altitudes, where conſequently the refractions muſt have been very different. But though this method may be liable to great inconveniences, the principal of which is, that many parts of the heavens are not ſufficiently ſtored with ſmall ſtars to give us an opportunity to meaſure from them, yet the advantages are not leſs remarkable. Thus we ſee that it enabled me to diſtinguiſh the quantity and direction of the motion of this Comet in a ſingle day (from the 18th to the 19th of March) to a much greater degree of exactneſs than could have been done in ſo ſhort a time by a ſector or tranſit inſtrument; nay even an hour or two, we ſee, were intervals long enough to ſhew that it was a moving body, and conſequently, had its ſize not pointed it out as a Comet, the change of place, though ſo trifling as 2¼ ſeconds *per* hour, would have been ſufficient to occaſion the diſcovery. A gentleman very well known for his remarkable ſucceſs in detecting Comets * ſeems to be well aware of the difficulty to diſcover a motion in a heavenly body by the common methods when it is ſo very ſmall; for in a letter he favoured me with, ſpeaking of the Comet, he ſays: "Rien n'etoit plus difficile que de la "reconnoître et je ne puis pas concevoir comment vous avés pu

* Monſ. MESSIER.

"revenir

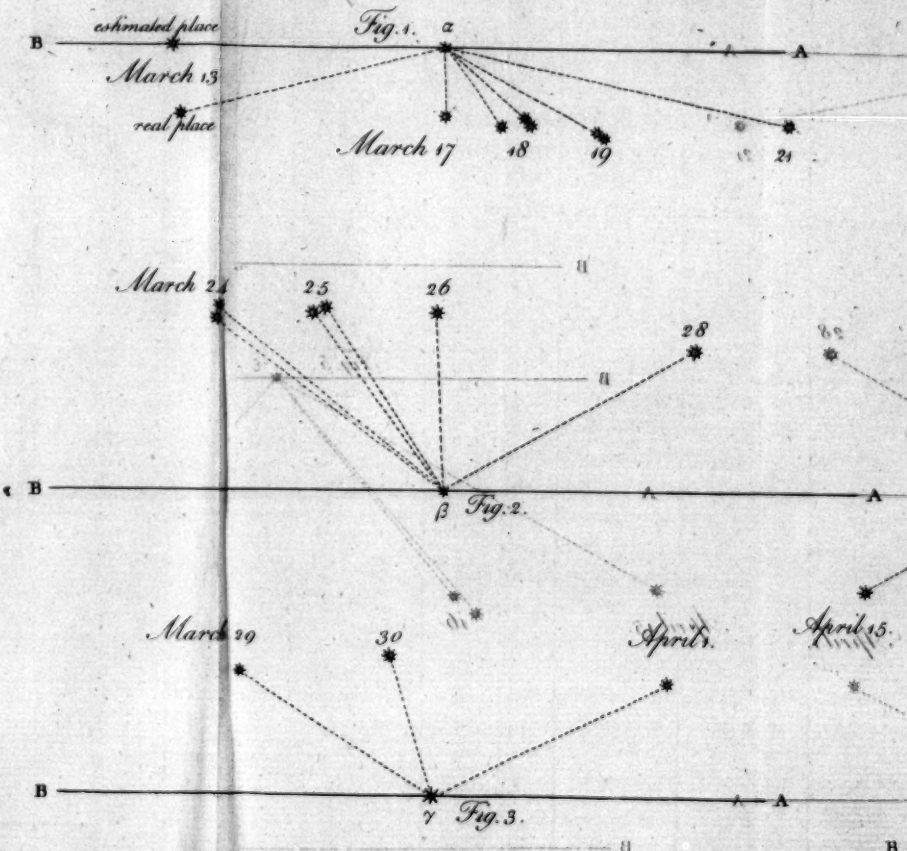
" revenir pluſieurs fois ſur cette étoile ou Comète; car abſolu-
" ment il a fallu l'obſerver pluſieurs jours de ſuite pour s'ap-
" perçevoir qu'elle avoit un mouvement."

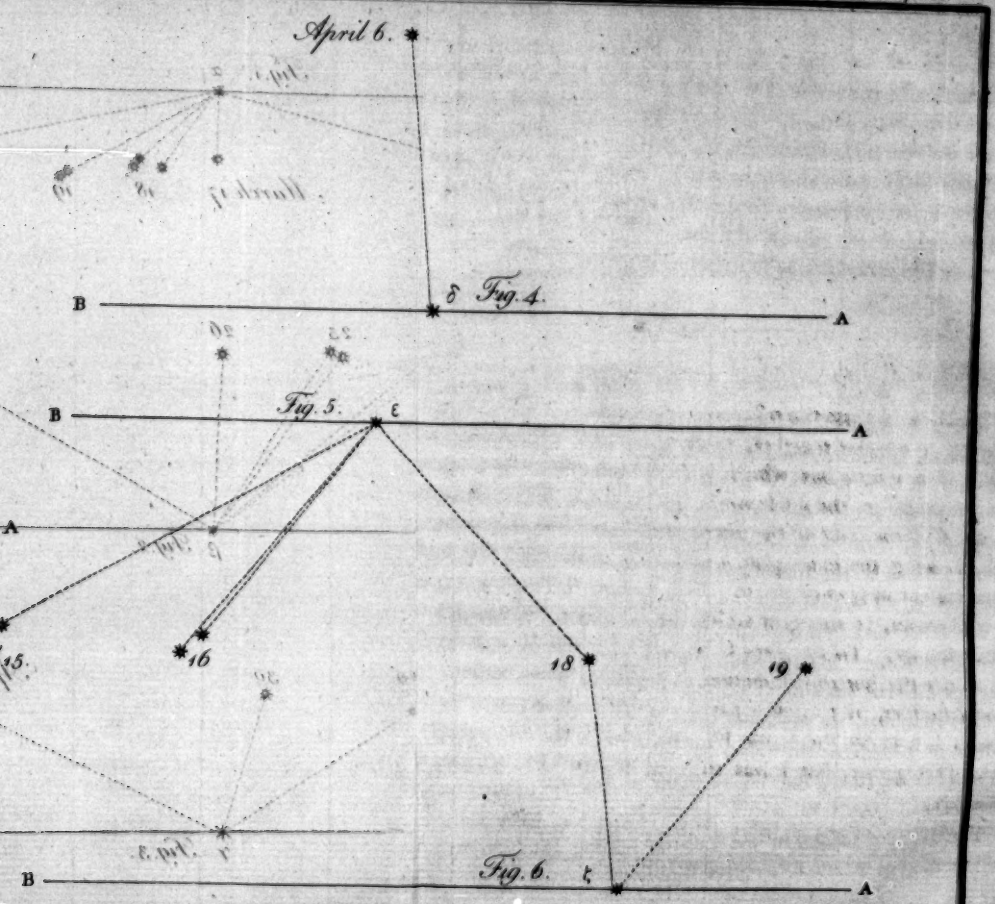
I need not ſay that I merely point this out as a temporary advantage in the method I have taken; for as ſoon as we can have regular, conſtant, and long continued obſervations by fixed inſtruments, the excellence of them is too well known to ſay any thing upon that ſubject: for which reaſon I failed not to give immediate notice of this moving ſtar, and was happy to ſurrender it to the care of the Aſtronomer Royal and others, as ſoon as I found they had begun their obſervations upon it.

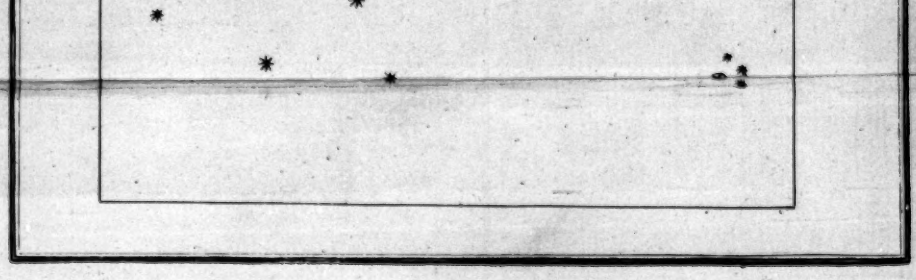
Deſcription of a micrometer for taking the angle of poſition.

FIG. I. Repreſents the micrometer incloſed in a turned caſe of wood, as it is put together, ready to be uſed with the teleſcope. A is a little box which holds the eye-glaſs. B is the piece which covers the inſide work, and the box A is ſcrewed into it. C is the body of the micrometer containing the braſs work, ſhewing the index plate *a* projecting at one ſide, where the caſe is cut away to receive it. D is a piece, having a ſcrew *b* at the bottom, by means of which the micrometer is faſtened to the teleſcope. To the piece C is given a circular motion, in the manner the horizontal motion is generally given to Gregorian reflectors, by the lower part going through the piece D, where it is held by the ſcrew E, which keeps the two pieces C and D together, but leaves them at liberty to turn upon each other.

Fig. II. Is a ſection of the caſe containing the braſs work, where may be obſerved the piece B hollowed out to receive the


estimated place
Fig. 1.
α
B
A
March 13
real place
March 17
18
19
21
March 24
25
26
28
B
A
β
Fig. 2.
March 29
30
April 1.
April 15.
B
A
γ
Fig. 3.
B


April 6.
B
δ Fig. 4.
A
B
Fig. 5.
ε
A
A
16
18
19
B
Fig. 6.
ζ
A



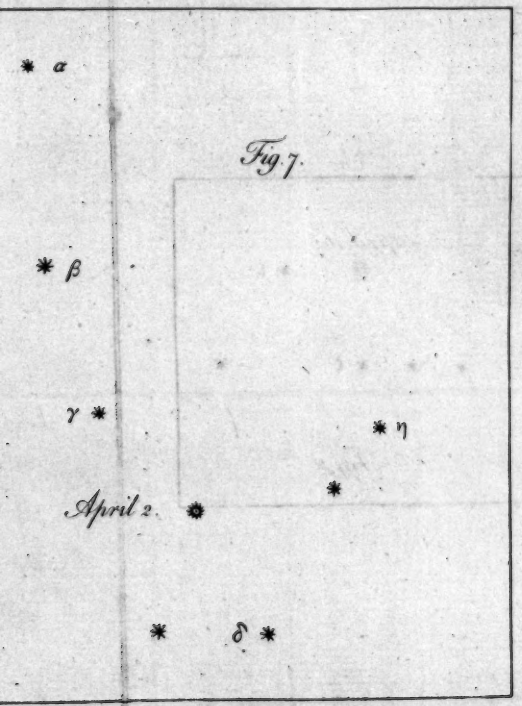


Fig. 7.

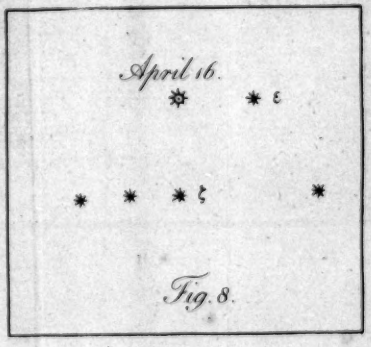


Fig. 8.

BASIRE Sc.

Fig. I.

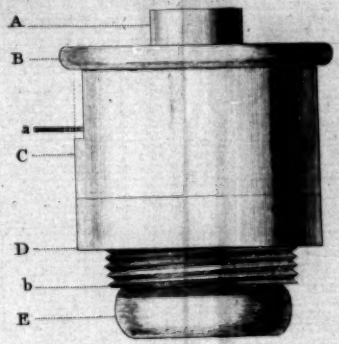


Fig. II.

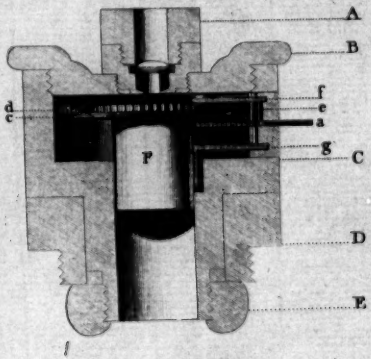


Fig. III.

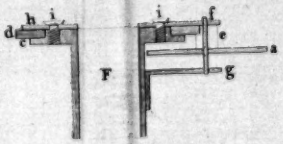
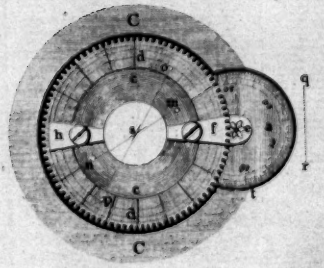


Fig. IV.



BASIRE SC.

box A, which consists of two parts inclosing the eye lens. This figure also shews how the piece C passes through D, and is held by the ring E: the brass work, consisting of a hollow cylinder, a wheel and pinion, and index plate, is there represented in its place. F is the body of the brass work, being a hollow cylinder with a broad rim *c* at the upper end; this rim is partly turned away to make a bed for the wheel *d*. The pinion *e* turns the wheel *d*, and carries the index plate *a*. One of its pivots moves in the arm *f*, screwed upon the upper part of *c*, which arm serves also to confine the wheel *d* to its place upon *c*. The other pivot is held by the arm *g* fastened to F.

Fig. III. Is a plan of the brass work. The wheel *d*, which is in the form of a ring, is laid upon the upper part of F or *c*, and held by two small arms *f* tnd *h*, screwed down to *e* with the screws *i*, *i*.

Fig. IV. Is a plan of the brass work. *d*, *d*, is the wheel placed upon the bed or socket of the rim of the cylinder *c*, *c*, and is held down by the two pieces *f*, *h*, which are screwed upon *e*, *e*. The piece *f* projects over the center of the index plate to receive the upper pivot of the pinion *m*, *n*, is the fixed wire fastened to *c*, *c*. *o*, *p*, the moveable wire fastened to the annular wheel *d*, *d*. The index plate *a* is divided into 60 parts, each sub-divided into two, and milled upon the edge. When the finger is drawn over the milled edge of the index plate from *q* towards *r*, the angle *m*, *s*, *o*, will open, and if drawn from *r* towards *q*, it will shut again. The case C, C, must have a sharp corner *t*, which serves as a hand to poin out the division on the index plate.

XXXIII. *A Letter from* Joseph Willard *to the Rev. Dr.* Maskelyne, *Astronomer Royal, concerning the Longitude of* Cambridge *in* New England.

Read July 5, 1781.

REV. SIR,

THE difference of meridians between Greenwich and Cambridge has been generally reckoned 4 h. 44′. This was what the late Dr. WINTHROP made use of; but I do not find that he determined it by actual observations, made by him at Cambridge, compared with corresponding ones, made at the Royal Observatory at Greenwich. It appears, that in 1769, at the time of the transit of Venus, the doctor was not entirely certain of the longitude of Cambridge. He mentioned 4 h. 44′ as near the truth; but for better fixing it, he gave several of his observations of the eclipses of Jupiter's satellites to be compared with those made at Greenwich; but there were too few corresponding ones to determine the point with precision; and as modern astronomers do not make absolute dependence upon the difference of meridians deduced from the eclipses of Jupiter's satellites, unless there has been a series of observations, both of immersions and emersions, I have wished to find some observations of solar eclipses and occultations of fixed stars by the moon, made at Cambridge, of which corresponding ones were made at Greenwich. I have met with no observations of occultations made by Dr. WINTHROP; but a solar

eclipſe was obſerved by him and ſeveral other gentlemen, at his houſe, Auguſt 5, 1766, at which I was preſent and aſſiſting, being then a reſident graduate at Harvard College: this eclipſe, Sir, I find that you obſerved at Greenwich. By your obſervations in the printed volume, a copy of which the Royal Society was ſo generous as to ſend to Harvard College, which was received the laſt ſpring, and for which the College is very grateful, I find the beginning of the eclipſe was ſeen by you at 5 h. 29′ 56″ P.M. and the end at 7 h. 11′ 27″ P.M. apparent time. At Dr. WINTHROP's houſe at Cambridge, lat. 42° 25′ N. the beginning of this eclipſe was obſerved at 11 h. 39′ 23″ A.M. and the end at 2 h. 45′ 9″ P.M. apparent time. Allowing for the ſpheroidal figure of the earth, and going through the parallactic calculations and deductions, I find the difference of meridians between Greenwich and Cambridge, by the obſervations of this eclipſe, to be 4 h. 44′ 22″.

In the tranſit of Venus, in 1769, the internal contact was obſerved by Dr. WINTHROP at 2 h. 47′ 30″ apparent time, and by Mr. HITCHINS, at the Royal Obſervatory, at 7 h. 28′ 57″ apparent time. Allowing the ſun's parallax on the day of the tranſit to be 8″,38, I find by calculation from theſe obſervations, that the difference of meridians between Greenwich and Cambridge is 4 h. 44′ 12″. The reaſon of my taking Mr. HITCHINS's obſervation is, your ſaying, that the teleſcope which he uſed was much ſuperior to all the others which were made uſe of at that day at the Obſervatory; and to its greater excellence and diſtinctneſs you attribute the difference of 26″ by which Mr. HITCHINS ſaw the internal contact before you. There ſeems to be the greater propriety, when comparing the American obſervations of that phenomenon with thoſe made at Greenwich, to take that where the obſerver was peculiarly

aided by the distinctness of his telescope, because the sun was very near the horizon with you, while with us the altitude was great, and the atmosphere exceeding clear. Taking the mean between the deduction made from the observations of the internal contact of Venus, and of the beginning and ending of the above solar eclipse, the difference of meridians between Greenwich and Cambridge is 4 h. 44′ 17″.

I find, Sir, in a letter from you to Dr. SMITH of Philadelphia, Dec. 26, 1769, that by the observations of the eclipses of Jupiter's satellites made at Norriton you determined the difference of meridians between Greenwich and Norriton to be 5 h. 1′ 0′ 35″. If we subtract 52″, the difference of meridians between Philadelphia and Norriton, gotten, agreeably to your request, by terrestrial measurement, we find the difference of meridians between Greenwich and Philadelphia to be 5 h. 0′ 43″, which is the same that it appears to be by the immersions and emersions of Jupiter's first satellite observed at Philadelphia, corrected in the same manner you corrected the observations for Norriton, which is 8″ more than Dr. EWING's determination. By observations of the transit of Mercury in 1769, made at Cambridge and Philadelphia, the difference of meridians between those two places appears by the external contact to be 16′ 02″, by the internal 16′ 28″; the mean 16′ 30″ subtracted from 5 h. 0′ 43″ leaves 4 h. 44′ 13″ for the difference of meridians between Greenwich and Cambridge deduced in this way, which, though not direct, may yet be considered as an evidence of some weight to prove, that the difference is more than 4 h. 44′, and that 4 h. 44′ 17″ may be very near the truth. This is the difference that I at present take, when I make use of tables fitted to the meridian of Greenwich; but I should be still glad of more

correſponding obſervations to aſcertain this point. June 24, 1778, there was a ſolar eclipſe, viſible both at Greenwich and Cambridge. The beginning of this eclipſe was obſerved at Cambridge by the late Dr. WINTHROP at 9 h. 6′ 20″ A.M. and the end at 11 h. 37′ 22″ A.M. apparent time. If the atmoſphere favoured your obſerving it at Greenwich, I ſhould be extremely obliged to you, if you would communicate to me your obſervations. I ſhall alſo be happy to know the time of the beginning of the ſolar eclipſe of the 23d of next April at Greenwich.

I now, Sir, beg leave to communicate to you ſome obſervations of the ſolar eclipſe of the 27th of laſt October. At Beverly, lat. 42° 36′ N. I carefully aſcertained the going of my clock, by equal altitudes of the ſun's upper and lower limb, for ſeveral days preceding the eclipſe, and on the day when it happened, conſtantly applying the equation for the change of declination. October 25th, when the ſun's center paſſed the meridian, it was by the clock 11 h. 59′ 11″; on the 26th, 11 h. 58′ 15″; and on the 27th, 11 h. 57′ 18″.

Two gentlemen obſerved with me; the rev. Mr. CUTLER of Ipſwich, and the rev. Mr. PRINCE of Salem. Mr. CUTLER and I were each furniſhed with a reflecting teleſcope made by JAMES MANN of London, one magnifying 34, and the other 45 times. Mr. PRINCE had an achromatic refractor magnifying 43 times. The times of obſervation are as follow:

		By the clock.	Reduced to app. time.
		h. ′ ″	h. ′ ″
Beginning of eclipſe obſerved by	Mr. CUTLER,	10 59 2	11 1 42 A.M.
	Mr. PRINCE,	10 59 6	11 1 46
	Myſelf,	10 59 8	11 1 48
End of eclipſe obſerved by	Mr. CUTLER,	1 38 37	1 41 23 P.M.
	Mr. PRINCE,	1 38 43	1 41 29
	Myſelf,	1 38 40	1 41 26

I had no micrometer to measure the lucid part of the sun in its least state, and thereby determine the error of MAYER's tables in latitude at the middle of this eclipse. But according to the observations of Professor WILLIAMS, Dr. WINTHROP's successor, the error was not great. He and several assistants observed this eclipse at Long Island in Penobscot Bay. The latitude of the place of observation he found to be 44° 17′ 7″ N. He observed the beginning of the eclipse at 11 h. 11′ 8″ A.M. and the end at 1 h. 50′ 25″ P.M. apparent time. He was furnished with an excellent DOLLOND's micrometer, with which he measured the sun's diameter on the morning of the eclipse, and the lucid parts many times during its continuance. By his observations, compared with those made at Beverly, I find the difference of meridians between Beverly and Long Island to 8′ 4″ in time. The time of the greatest obscuration was at 12 h. 30′ 22″, when the lucid part of the sun was 8″ on the lower limb. The sun's semi-diameter according to observation was 16′ 8″,7; the moon's horizontal semi-diameter, according to the tables, 16′ 23″,8; the augmentation of her semi-diameter, agreeably to her altitude, 9″,5; the sum of the visible semi-diameters of the sun and moon therefore 32′ 42″. The lucid part of the sun 8″ being subtracted from 32′ 17″,4, the sun's diameter, leaves 32′ 9″,4 for the eclipsed part of the sun, which subtracted from 32′ 42″ leaves 32″,6 for the least distance of the centers of the sun and moon. The visible angle of the moon with the sun was, I find, 15° 54′ 54″, and her motion from the sun in her visible orbit 24″,5 in one minute; the visible ecliptic conjunction therefore was at 12 h. 29′ 57″, and the distance of the centers of the sun and moon 34″. The moon's parallax in latitude from the sun was then 49′ 15″,8 S. which added to 34″ gives 49′ 49″,8 N. equal to the moon's lati-

tude by obſervation. By MAYER's tables her latitude was then 49′ 39″,4, by which it appears, that the error of theſe tables in latitude, at the middle of the eclipſe, by the Penobſcot obſervation, was −10″4. The error in longitude, taking the mean between that of the beginning and end, I find to be −9″. The determination of theſe errors is upon the ſuppoſition that Beverly is 4 h. 42′ 59″ W. of Greenwich, which, I believe, very near the truth. Beverly appears by obſervation to be 1′ 18″ in time E. of Cambridge, which ſubtracted from 4 h. 44′ 17″ leaves 4 h. 42′ 59″.

I hope, Sir, no umbrage will be taken at my writing to you, on account of the political light in which America is now viewed by Great Britain. I think political diſputes ſhould not prevent communications in matters of mere ſcience; nor can I ſee how any one can be injured by ſuch an intercourſe.

I am, &c.

Beverly in Maſſachuſetts,
February 16, 1781.

AN

ACCOUNT

OF SOME

THERMOMETRICAL EXPERIMENTS;

CONTAINING,

I. Experiments relating to the cold produced by the evaporation of various fluids, with a method of purifying ether.
II. Experiments relating to the expanſion of mercury.
III. Deſcription of a thermometrical barometer.

By TIBERIUS CAVALLO, F.R.S.

Who was nominated by the Preſident and Council to proſecute Diſcoveries in Natural Hiſtory, purſuant to the Will of the late HENRY BAKER, Eſq. F.R.S.

Read at the ROYAL SOCIETY, June 28, 1781,

AN

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THERMOMETRICAL EXPERIMENTS;

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I. Experiments relating to the cold produced by the evaporation of various fluids, with a method of purifying ether.
II. Experiments relating to the expansion of mercury.
III. Description of a thermometrical barometer.

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Who was nominated by the President and Council to prosecute Discoveries in Natural History, pursuant to the Will of the late HENRY BAKER, Esq. F.R.S.

Read at the ROYAL SOCIETY, June 24, 1781.

AN ACCOUNT, &c.

IT is at preſent well known, that by the evaporation of various fluids a ſenſible degree of cold is produced; and that by the evaporation of ether, which is the moſt volatile fluid we are acquainted with, water may be congealed, and the thermometer may be brought ſeveral degrees below the freezing point. But as various thermometrical experiments, which I lately made, have exhibited ſome new phenomena, and as I have contrived an eaſy and pleaſing method of freezing a ſmall quantity of water in a ſhort time, and in every climate; I think it not improper to give an account of theſe things in the firſt part of this lecture.

My firſt experiments were intended to diſcover, if poſſible, a fluid cheaper than ether, by the evaporation of which a degree of cold ſufficient for ſome uſeful purpoſe might be generated. But in this my expectation was diſappointed, as I found that ether was incomparably ſuperior to any other fluid, as the cold it produced was ſeveral degrees greater than that occaſioned by any other of the moſt volatile fluids whatever. Being, therefore, obliged to uſe ether, I endeavoured to contrive a method, by which the leaſt poſſible quantity of it might be waſted in the production of a degree of cold ſufficient to freeze water, and in this I met with ſucceſs. But before we come to the deſcription of this method, I

ſhall briefly relate ſome obſervations made on the cold produced by the evaporation of other fluids beſides ether.

In a room, the temperature of which was 64° according to FAHRENHEIT's thermometer, and in which the air was gently ventilated, I obſerved the effects produced by various fluids when thrown upon the ball of a thermometer. The ball of this thermometer was quite detached from the ivory piece upon which the ſcale was engraved. The various fluids were thrown upon the thermometer through the capillary aperture of a ſmall glaſs veſſel, ſhaped like a funnel, and care was taken to throw them ſo ſlowly upon the bulb of the thermometer, that a drop might now and then fall from the under part of it; except when thoſe fluids were uſed, which evaporate very ſlowly, in which caſe it was ſufficient to keep the ball of the thermometer only moiſt, without any drop falling from it. During the experiment the thermometer was kept turning very gently round its axis, in order that the fluid uſed might fall upon every part of its bulb. This method I find to anſwer much better than that of dipping the ball of the thermometer into the fluid and removing it immediately after, or that of wetting the thermometer with a feather. The evaporation, and conſequently the cold produced by it, may be increaſed by ventilation, *viz.* by blowing with a pair of bellows upon the thermometer; but this was not uſed in the following experiments, becauſe it is not eaſily performed by one perſon, and alſo becauſe it occaſions very uncertain reſults.

With the above deſcribed method I began to examine the effects of water, and found, that the thermometer was brought down to 56°, *viz.* 8° below the temperature of the room in which the experiment was made, and of the water employed.

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This effect was produced in about two minutes time, after which a larger continuation did not bring the mercury lower.

By means of ſpirit of wine the thermometer was brought down to 48°, which is only 16° below the temperature of the room, and of the ſpirit employed. When the ſpirit of wine is highly rectified, the cold produced by its evaporation is certainly greater than when it is of the common ſort; but the difference is not ſo great as one, who never tried the experiment, might expect. The purer ſpirit produces the effect much quicker.

Uſing various other fluids, which were either compounds of water and ſpiritous ſubſtances, or pure eſſences, I found that the cold produced by their evaporation was generally in ſome intermediate degree between the cold produced by the water and that produced by the ſpirit of wine.

Spirit of turpentine brought the thermometer only 3° lower than the temperature of the room; but olive oil and other oils, which evaporate either very ſlowly or not at all, did not ſenſibly affect the thermometer.

Willing to obſerve how much electrization could increaſe the evaporation of ſpirit of wine, and conſequently the cold produced by it, I put the tube containing the ſpirit into an inſulating handle, and connected it with the conductor of an electrical machine, which was kept in action whilſt the experiment was performed; by theſe means the thermometer was brought down to 47°. Having tried the three mineral acids I found, that inſtead of cooling they heated the thermometer, which effect I expected; ſince it is well known, that thoſe acids attract the water from the atmoſphere, and that heat is produced by the combination of water and any of them. The vitriolic acid, which was very ſtrong and tranſparent, raiſed the

thermometer to 102°; the ſmoaking nitrous acid raiſed it to 72°; and the marine acid raiſed it to 66°; the temperature of the room, as well as of the acids, being 64°, as mentioned above.

The apparatus which I contrived for the purpoſe of uſing the leaſt poſſible quantity of ether in freezing water, &c. conſiſts in a glaſs tube, terminating in a capillary aperture, which tube is to be fixed upon the bottle that contains the ether. Fig. 1. of the annexed drawing exhibits ſuch a tube, round the lower part of which, *viz.* at A ſome thread is wound, in order to let it fit the neck of the bottle. When the experiment is to be made, the ſtopper of the bottle containing the ether is removed, and the above mentioned tube is fixed upon it. The thread round this tube ſhould be moiſtened a little with water or ſpittle before it is fixed on the bottle, in order to prevent more effectually any eſcape of ether between the neck of the bottle and the tube. Then holding the bottle by its bottom FG (fig. 2.) and keeping it inclined as is ſhewn in the figure, the ſmall ſtream of ether iſſuing out of the aperture D of the tube DE, is directed upon the ball of the thermometer, or upon a tube containing water or other liquor that is required to be congealed.

Ether being very volatile, and having the remarkable property of increaſing the bulk of air, does not require any aperture, through which the air might enter the bottle, in proportion as the ether goes out: the heat of the hand is more than ſufficient to force the ether in a ſtream from the aperture D.

After this manner, throwing the ſtream of ether upon the ball of a thermometer in ſuch quantity as that a drop of ether might now and then, for inſtance every 10 ſeconds, fall from the under part of the thermometer, I have brought the mercury down to 3°, *viz.* 29° below the freezing point, when the atmoſphere

atmoſphere was ſomewhat hotter than temperate, and that without blowing upon the thermometer.

When the ether is very good, *viz.* is capable of diſſolving elaſtic gum, and the thermometer has a ſmall bulb, not above twenty drops of ether are required to produce this effect, and about two minutes of time; but when the ether is of the common ſort, a greater quantity of it, and a longer time, is neceſſary to be employed, though at laſt the thermometer is brought down very nearly as low by this as by the beſt ſort of ether.

In order to freeze water by the evaporation of ether, I take a thin glaſs tube about four inches long and about one-fifth of an inch in diameter, hermetically cloſed at one end, and put a little water in it, ſo as to fill about half an inch length of it, as is ſhewn at CB in the figure. Into this tube a ſlender wire H is alſo introduced, the lower extremity of which is twiſted in a ſpiral manner, and ſerves to draw up the ice, when formed. Things being thus prepared, I hold the glaſs tube by its upper part A with the fingers of the left hand, and keep it continually and gently turning round its axis, firſt one way, and then the contrary; whilſt with the right hand I hold the phial containing the ether in ſuch a manner as to direct the ſtream of ether on the outſide of the tube, and a little above the ſurface of the water in it. The capillary aperture D ſhould be kept almoſt in contact with the ſurface of the tube that contains the water. Continuing this operation for two or three minutes, the water will be froze as it were in an inſtant; ſince it will appear to become opaque at the bottom B, and the opacity will aſcend at C in leſs than half a ſecond of time, which exhibits a beautiful appearance. This congelation, however, is only ſuperficial, and in order to congeal the whole quantity of wa-

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ter, the operation must be continued one or two minutes longer; after which the wire H will be found to be kept very tight by the ice. Now the bottle with the ether is left upon a table or other place, and to the outside of the glass tube the hand must be applied for a moment, in order to soften the surface of the ice, which adheres very firmly to the glass, and then pulling the wire H out of the tube, a solid and hard piece of ice will come out, fastened to its spiral extremity.

Instead of the wire H sometimes I put a small thermometer into this tube so as to have its bulb immersed in the water. With this thermometer I have observed a very remarkable phenomenon, which seems to be not explicable in the present state of knowledge concerning heat and cold. This is, that water will freeze in the winter with a less degree of cold than it will in the summer, or when the weather is hotter: for instance, in the winter the water in the tube AB will freeze when the thermometer is about 30°; but in the summer, or even when the temperature of the atmosphere is about 60°, the quicksilver in the thermomer must be brought ten or fifteen, or even more, degrees below the freezing point, before the water which surrounds the said thermometer will be converted into ice, even superficially; hence it appears, that in the summer time a greater quantity of ether and longer time is required to freeze a given quantity of water than in the winter, not only because then a greater degree of heat is to be overcome, but principally because in the summer a much greater degree of cold must be actually produced before the water that is kept in it will assume a solid form. When the temperature of the atmosphere has been about 40°, I have froze a quantity of water with an equal weight of good ether, but at present,

being ſummer, between two and three times the quantity of the ſame ether muſt be uſed to produce the ſame effect.

There ſeems to be ſomething in the air which, beſides heat, interferes with the freezing of water, and perhaps of all fluids, though I cannot ſay from experience whether the above mentioned difference between the freezing of water in winter and ſummer, takes place with other fluids, as milk and other animal fluids, oils, wines, &c.

The proportion between the quantity of the ether and of the water that may be frozen by it, ſeems to vary according to the quantity of water; for a larger quantity of water ſeems to require a proportionably leſs quantity of ether than a ſmaller quantity of water, ſuppoſing that the water is contained in cylindrical glaſs veſſels; for I have not tried whether a metal veſſel inſtead of a glaſs one, and whether ſome other ſhape beſides the cylindrical, might not facilitate the congelation. In the beginning of the ſpring I froze about a quarter of an ounce of water with nearly half an ounce weight of ether, the apparatus being larger, though ſimilar to that deſcribed above.

Now as the price of ether, ſufficiently good for the purpoſe, is generally between eighteen pence and two ſhillings *per* ounce, it is plain, that with leſs than two ſhillings a quarter of an ounce of ice, or ice cream, may be made in every climate, and at any time, which may afford great ſatisfaction to thoſe perſons, who living in places where no natural ice is to be had, never ſaw or taſted any ſuch delicious refreſhments.

When a ſmall piece of ice, for inſtance, of about ten grains in weight, is wanted, the neceſſary apparatus is very ſmall, and the expence of the ether not worth mentioning. I have a ſmall box, which is four inches and a half long, two inches broad, and one inch and a half deep, which contains all the apparatus

ratus neceſſary for this purpoſe, *viz.* a bottle capable of containing about one ounce of ether, two pointed tubes (in caſe that one ſhould break) a tube in which the water is to be frozen, and the wire. With the quantity of ether contained in this ſmall and very portable apparatus, the experiment, when carefully performed, may be repeated about ten times. A perſon who wiſhes to perform ſuch experiments in hot climates, and in places where ice is not eaſily procured, requires only a large bottle of ether, beſides the ſmall apparatus deſcribed above.

It is a known fact, that the moment a quantity of water becomes ice, a thermometer kept immerſed in it, riſes a few degrees, and accordingly this is obſerved in our experiment, *viz.* the mercury of the thermometer, which is immerſed in the water of the tube AB, will ſuddenly riſe, ſometimes as much as ten degrees, when the water becomes firſt opaque. Electrization increaſes very little the degree of cold produced by the evaporation of ether. Having thrown the electrified, and alſo the unelectrified, ſtream of ether upon the bulb of a thermometer, the mercury in it was brought down two degrees lower in the former than in the latter caſe.

As various perſons may, perhaps, be induced by this paper to repeat ſuch experiments, and as ether is a fluid which can with difficulty be preſerved, it may be uſeful to mention, that a cork confines ether in a glaſs bottle much better than a glaſs ſtopple, which it is almoſt impoſſible to grind ſo well as intirely to prevent the evaporation of ether. When a ſtopple, made very nicely out of a uniform and cloſe piece of cork, which goes rather tight, is put upon a bottle of ether, the ſmell of that fluid cannot be perceived through it; but I never ſaw a glaſs ſtopple that could produce the ſame effect. By open-

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ing the bottle very often, or by long keeping, the cork becomes looſe, in which caſe it muſt be changed; and thus ether, ſpirit of wine, or any fluid, excepting thoſe which corrode cork, may be preſerved.

I ſhall now deſcribe a method of purifying vitriolic ether, which is very eaſy and expeditious, though not very profitable: this method I learned of Mr. WINCH, Chemiſt, in the Hay-market. Fill about a quarter of a ſtrong bottle with common ether, and upon it pour about twice as much water, then ſtop the bottle, and give it a ſhake, ſo as to mix for a time the ether with the water. This done, keep the bottle without motion, and with the mouth downwards, till the ether is ſeparated from the water and ſwims over it, which requires not above three or four minutes of time; then open the bottle, and keeping it ſtill inverted, let the greateſt part of the water come out very gently; after this the bottle being turned with the mouth upwards, more water muſt be poured in it, and in ſhort the ſame operation muſt be repeated three or four times. Laſtly, all the water being ſeparated from the ether by decanting it with dexterity, the ether will be found to be exceedingly pure. By this means I have purified common vitriolic ether, which could not affect elaſtic gum, and have reduced it into ſuch a ſtate as that elaſtic gum was eaſily diſſolved by it. Indeed this purified ether appeared by every trial to be purer than I ever ſaw it, even when made after the beſt uſual method, and in the moſt careful manner. The only inconvenience attending this proceſs is, that a vaſt quantity of ether is loſt. Not above three or four ounces of a pound of common ether remain after the purification. As the greateſt part of the ether is certainly mixed with the water that is uſed in the proceſs, it may perhaps be worth while to put that water into a retort, and

to diſtil the ether from it, which muſt come ſufficiently pure for common uſe.

It is commonly believed, that water combines with the pureſt part of ether, when thoſe two fluids are kept together; whereas, by the above deſcribed proceſs, the contrary is eſtabliſhed: perhaps when ether is kept in contact with water for a long time, the pureſt part of it may appear to be loſt, becauſe the ether may be combined with, and may retain ſome water in itſelf, at the ſame time that the water combines with and retains ſome ether, whereas the caſe may be different when the ether is quickly waſhed in water, and is immediately after ſeparated from it: but in reſpect to this I have yet not made any experiments, ſo as to be able to decide the matter.

II. *Experiments relating to the expanſion of mercury.*

THE difficulty and uncertainty attending the various methods hitherto propoſed for inveſtigating the expanſion of quickſilver, or its increaſe of bulk when rarified by a given degree of heat, determined me to contrive ſome method by which this purpoſe might be effected with more certainty and preciſion. After various experiments I hit upon the following method, which to me ſeems both new and capable of great accuracy, though in this I may be deceived.

Firſt, having blown a ball to a capillary tube, ſuch as are commonly uſed for thermometers, I weighed it, and found that this empty thermometer was equal to 79,25 grains. This empty glaſs previous to its being weighed was rendered as perfectly

ſectly clean as poſſible, which is a neceſſary precaution in this experiment, which depends upon a very great accuracy of weight. Then I introduced ſome mercury into the ſtem of this thermometer, taking care that none of it entered the ball, and, by adapting a ſcale of inches to the tube, obſerved that 4,3 inches length of the tube was filled with the mercury. The thermometer was now weighed again, and from this weight, the weight of the glaſs found before being ſubtracted, the remainder, *viz.* 0,24 gr. ſhewed the weight of ſo much quickſilver as filled 4,3 inches of the tube. Now the ball of the thermometer, and alſo part of the tube, were intirely filled with quickſilver: then, in order to find out the weight of the mercury contained in it, the thermometer was weighed for the laſt time, and from this weight the weight of the glaſs being ſubtracted, the remainder, *viz.* 3205 grs. ſhewed the weight of the whole quantity of quickſilver contained in the thermometer.

By compariſon with a graduated thermometer in hot and cold water, I made a ſcale to the new thermometer according to FAHRENHEIT's, and by applying a ſcale of inches found, that the length of 20° in this ſcale was equal to 1,33 inch. But 0,24 gr. was the weight of ſo much mercury as filled 4,3 inches length of the tube; therefore, by the rule of proportion it will be found, that the weight of ſo much quickſilver as fills 1,33 inch of the tube, *viz.* the length of 20° is equal to 0,0742 gr. nearly, and that the weight of ſo much quickſilver as fills the length of the tube that is equivalent to one degree, is equal to 0,00371 gr. Now it is clear, that the weight of the whole quantity of quickſilver contained in the thermometer is to the weight of ſo much quickſilver as fills the length of one degree in the tube, as the bulk of the whole quantity of quick-

ſilver in a given degree of heat, to the increaſe of bulk, that the ſame whole quantity of quickſilver acquires when heated of but 1°; *viz.* 32,05 grs. is to 0,00371 gr. as 1 is to 0,00011+; ſo that by this experiment it appears, that 1° of FAHRENHEIT's thermometer increaſes the bulk of mercury not above $\frac{1}{9000}$ths parts. In this proceſs a ſmall deviation from mathematical exactneſs is occaſioned by the ſmall difference of weight between the quickſilver of the tube when firſt weighed and when it is afterwards heated to 1°; but by an eaſy calculation it will be found, that this difference is ſo exceedingly ſmall as not to be perceived by our exacteſt weighing and meaſuring inſtruments.

For clearneſs ſake I ſhall ſubjoin the calculation of the above related experiments, diſencumbered from words. Here the decimals are not computed to a very large number, that being unneceſſary for this purpoſe.

Weight of the glaſs, - - - -	79,25 grs.
Weight of ſo much quickſilver as filled 4,3 inches length of the tube, - - -	0,24 grs.
Weight of the whole quantity of quickſilver contained in the thermometer, - -	32,05 grs.
Length of the tube equal to 20°, - -	1,33 inch.

4,3 : 0,24 :: 1,33 : 0,0742 = 20°

20° : 0,0742 :: 1 : 0,00371

32,05 : 0,00371 :: 1 : 0,00011 + = to the expanſion occaſioned by one degree of heat.

Having repeated this experiment with other thermometers, and by ſimilar calculations, each proceſs gave a reſult little different from the others, which irregularity is certainly owing to the imperfection of my ſcales, which are not of the niceſt ſort: but taking a mean of various experiments it appears,

that

that 1° of heat, according to FAHRENHEIT's thermometer, increaſes the bulk of a quantity of quickſilver of $\frac{1}{10000}$ parts, viz. if the bulk of a quantity of quickſilver in the temperature of 50° is equal to 100,000 cubic inches, the bulk of the ſame quantity of quickſilver in the temperature of 51° will be equal to 100,009 cubic inches.

It is almoſt ſuperfluous to mention, that the cavity of the tubes employed for theſe experiments, muſt be perfectly uniform throughout. The ſcales to be uſed for this method ſhould be ſo exact as to be turned by the hundredth part of a grain when charged with about half an ounce weight.

From theſe obſervations the method of graduating, or of determining the length of a degree in a new thermometer, is eaſily deduced, the only requiſites for the calculation being the weight of a quantity of quickſilver, which fills a known length of the tube, and the weight of the whole quantity of quickſilver contained in the thermometer when filled. Suppoſe, for inſtance, that in making a new thermometer it be found, that the weight of ſo much quickſilver as fills five inches length of the tube is equal to ten grains, and that the weight of the whole quantity of quickſilver contained in the thermometer weighs 300 grains. It is plain, that if the whole quantity of quickſilver weighs 300 grs. $\frac{1}{10000}$ parts of it muſt weigh 0,027 gr. But the weight of ſo much mercury as fills five inches of the tube is equal to 10 grains; therefore, 0,027 gr. weight of quickſilver muſt fill 0,0133 inch of the tube, and this is equal to the length of 1°, or the double, treble, &c. of it is equal to two, three, &c. degrees.

By this means the ſcale may be made; that is, it may be divided into degrees, but the numbers cannot be added to them, without finding which of thoſe degrees correſponds with the

freezing point or boiling point. Either the point of boiling or freezing may be found by experiment, or any other point may be ascertained by comparison with another thermometer, and then the other degrees are nominated accordingly.

III. *Description of a thermometrical barometer.*

THE determination of the various degrees of heat shewn by boiling water under different pressures of the atmosphere, has been attempted by various persons, but it was lately completed by the accurate and numerous experiments of Sir GEORGE SHUCKBURGH, member of this Society. His valuable paper is inserted in the LXIXth vol. of the Phil. Transf. Upon considering this paper, I thought it possible to construct a thermometer with proper apparatus, which, by means of boiling water, might indicate the various gravity of the atmosphere, viz. the height of the barometer. This thermometer, together with the suitable apparatus, might, I thought, be packed into a small and very portable box, and I even flattered myself, that with such an instrument the heights of mountains, &c. might perhaps be determined with greater facility than with the common portable barometer. My expectations are far from having been disappointed, and although the instrument which I have hitherto constructed has various defects, I have, however, thought of some expedients which will undoubtedly render it much more perfect; I shall then present to this Society a more particular account of it, and also of the experiments which I intend to make with it. The instrument in its present state consists of a cylindrical tin vessel, about two inches in diameter and five inches high, in which vessel the water is con-

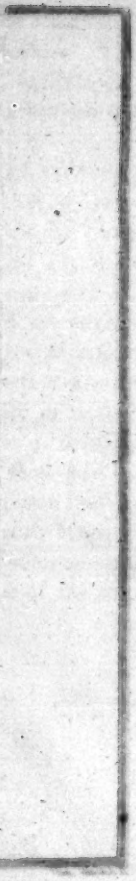
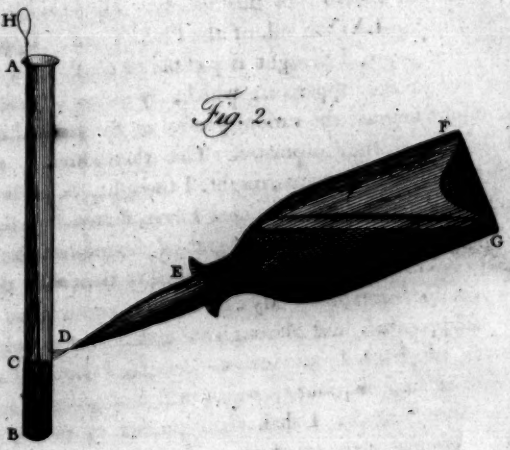


Fig. 1.

Fig. 2.



contained, which may be made to boil by the flame of a large wax candle. The thermometer is faſtened to the tin veſſel in ſuch a manner as that its bulb may be about one inch above the bottom. The ſcale of this thermometer, which is of braſs, exhibits on one ſide of the glaſs tube a few degrees of FAHRENHEIT's ſcale, *viz.* from 200° to 216°. On the other ſide of the tube are marked the various barometrical heights, at which the boiling water ſhews thoſe particular degrees of heat which are ſet down in Sir G. SHUCKBURGH's table. With this inſtrument the barometrical height is ſhewn within one-tenth of an inch. The degrees of this thermometer are ſomewhat longer than one-ninth of an inch, and conſequently may be ſubdivided into many parts, eſpecially if a nonius is uſed. But the greateſt imperfection of this inſtrument ariſes from the ſmallneſs of the tin veſſel, which does not admit a ſufficient quantity of water: and I find, that when a thermometer is kept in a ſmall quantity of boiling water, the quickſilver in its ſtem does not ſtand very ſteady, ſometimes riſing or falling even half a degree; but when the quantity of water is ſufficiently large, for inſtance is ten or twelve ounces, and is kept boiling in a proper veſſel, its degree of heat under the ſame preſſure of the atmoſphere is very ſettled.

contained, which may be made to boil by the flame of a large wax candle. The thermometer is fastened to the tin vessel in such a manner as that its bulb may be about one inch above the bottom. The scale of this thermometer, which is of brass, exhibits on one side of the glass tube a few degrees of FAHRENHEIT's scale, viz. from 200° to 212°. On the other side of the tube are marked the various barometrical heights, at which the boiling water shews those particular degrees of heat which are set down in Sir G. SHUCKBURGH's table. With this instrument the barometrical height is shewn within one-tenth of an inch. The degrees of this thermometer are somewhat longer than one-ninth of an inch, and consequently may be subdivided into many parts, especially if a nonius is used. But the greatest imperfection of this instrument arises from the smallness of the tin vessel, which does not admit a sufficient quantity of water: and I find, that when a thermometer is kept in a small quantity of boiling water, the quicksilver in its stem does not stand very steady, sometimes rising or falling even half a degree; but when the quantity of water is sufficiently large, for instance is ten or twelve ounces, and is kept boiling in a proper vessel, its degree of heat under the same pressure of the atmosphere is very certain.

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Page. Line.

16. 5. *for* Goan *read* Jean
— 15. *for* ℥iſſ. *read* ʒiſſ.
17. 1. *for* of vitriol *read* of tartar of vitriol
18. 7. *for* erica, formed, *read* erica-formed
— 19. *for* wetted *read* melted
21. 13. *for* clays *read* clay
23. 14. *for* ℥vij. *read* ʒvij.
24. 1. *for* ℥v. *read* ʒv.
26. 6. *for* dale *read* deal
28. 12. *for* ſubſtance *read* ſubſtances
30. 20. *for* tin *read* fire
33. laſt line, *omit the word* than
34. 23. *after* iron *inſert* ore.

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478. 1. Journal of the Weather at Senegambia, &c. *ſhould be* Journal of the Weather at Senegal, [For Senegambia is the whole province, the river Gambia included, in which river the weather muſt have been quite different at this period from what it was at Senegal, becauſe the diſorder did not rage there.
483. laſt line, *read* that the wind in the ſame month in the river Gambia, &c.
490. 6. *for* blank *read* planked

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Page.	Line.	
334.	5.	*for* fits *read* fets
343.	20.	*for* flown *read* flowed.
363.	3.	of the note *, *for* after a labour *read* after a natural labour.
—	9.	——— *for* eight inches *read* five inches.
383.	13.	*for* then *read* there
417.	1.	art. 23. *for* (art. 27.) *read* (art. 28.)
423.	5.	art. 28. *for* (art. 24.) *read* (art. 23.)
425.		last line but two, *for* $1-\overline{p}$ *read* $1-2p^2$

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